I'

1776

HARVARD PSYCHOLOGICAL STUDIES

EDITED BY

HUGO MÜNSTERBERG

Volume III

CAMBRIDGE
HARVARD UNIVERSITY PRESS
1913

PREFACE

THE first two volumes of the Harvard Psychological Studies, containing thirteen hundred pages of reports on research in the Harvard Psychological Laboratory, appeared as independent books in the market. After publishing the second volume we became convinced that this was not the most desirable method. Such large books from one single laboratory could not possibly find a circulation as wide as that of the general psychological magazines. Hence it seemed more advantageous to print the papers in the regular archives and periodicals. Moreover, our volumes had to contain investigations which belonged to many different branches of psychology and which therefore naturally appealed to different groups of readers. In order to secure a single article, anyone interested had to obtain a volume in which most of the papers were outside of his field. But the chief objection was that the appearance in such a form greatly delayed the publication of the experiments. The manuscripts had to lie over until our standard volumes of six hundred pages were filled. For these and similar reasons we decided to give up the publication of the Harvard Psychological Studies.

Our first intention was henceforth to confine ourselves to publishing the Harvard laboratory reports in the various periodicals or as isolated monographs. This has been going on for several years. Yet this new situation made us feel increasingly the disadvantage of seeing the work of our students scattered; our publications lost the effect of unity. So we finally settled on a compromise scheme. We shall go on with publication in any journals which secure the quickest and widest distribution and which easily bring the various papers to the different groups of interested specialists. But at the same time we intend to collect reprints of these papers and bind them together from time to time in volumes which will not come to the book market, but which will be distributed through the University to psychological laboratories, scholarly libraries, and so on. We intend to edit such volumes at first in one hundred and fifty copies.

They will never contain anything which has not been previously published in periodicals or independently.

This third volume is the first effort in the new direction. It is incomplete, as reprints could now be obtained only from a limited number of publications. Half of those which we have collected here belong to human psychology and half to animal psychology. I have arranged both groups simply in alphabetical order. In future we shall always secure the reprints at once, and it is probable that such volumes will be sent out at not very long intervals.

The research work of the Harvard Laboratory in the field of animal psychology was entirely under the supervision of Professor R. M. Yerkes; the work in human psychology was under my control, in coöperation with Professor E. B. Holt, until the year before last, and with Dr. H. S. Langfeld in the last two years.

HUGO MÜNSTERBERG.

HARVARD UNIVERSITY,
May, 1913.

TABLE OF CONTENTS

HUMAN PSYCHOLOGY

ANIMAL PSYCHOLOGY

TABLE OF CONTENTS

Monograph Supplements

Vol. XII
No. 3

January, 1910
Whole No. 50

THE

Psychological Review

EDITED BY

HOWARD C. WARREN
PRINCETON UNIVERSITY

JOHN B. WATSON
JOHNS HOPKINS UNIVERSITY

JAMES R. ANGELL
UNIVERSITY OF CHICAGO
(*Editor of the Psychological Monographs*)

Studies in Melody

BY

W. Van Dyke Bingham

Instructor in Educational Psychology, Teachers College,
Columbia University

THE REVIEW PUBLISHING COMPANY
41 NORTH QUEEN ST., LANCASTER, PA.
AND BALTIMORE, MD.

WAVERLY PRESS
WILLIAMS & WILKINS COMPANY
BALTIMORE

PREFACE.

In the first portion of this monograph are presented the results of investigations made in the psychological laboratory of the University of Chicago during the years 1905-07. The experiments which form the basis of the remainder of the work were carried on during the year 1907-08 in the Harvard psychological laboratory.

To the directors of these two laboratories, Professor James Rowland Angell and Professor Hugo Münsterberg, the writer desires to express his gratitude for patient counsel and stimulating criticism. He wishes also to acknowledge his obligation to the fellow-students of experimental psychology, who, in the capacity of observers, made possible the prosecution of these studies.

To the investigations of Professor R. H. Stetson in the field of rhythm the writer owes the method of attack employed in studying the relationships of muscular movement to the melody experience; and the outline of a motor theory of melody with which the present study is brought to a close is obviously the outgrowth of suggestions from Professor Stetson's important publications. Indebtedness to Professor Max Meyer is likewise evident, and nowhere more plainly than in those passages which express disagreement with his views.

My controversy with Professor Meyer is in part made necessary because of what seems to me to be an equivocal use of the term 'tonal relationship' on his part; and lest a similar ambiguity creep in to vitiate the discussions of the following pages, I have taken pains in each instance to specify in which of its two common meanings the term "relationship" is used. Musicians speak of two tones as directly "related" when the ratios of their vibration-rates are so simple that one tone is found among the first five partials of the other, or, what amounts to the same thing, when the two tones belong to a major triad, the 'chord of nature.' The "feeling of relationship" is the

experience of coherence, of 'belonging-togetherness,' which characterizes the hearing of two successive tones of the sort described. The question as to what pairs of tones arouse this feeling of "relationship" must of course be answered not by an arbitrary definition but by reference to the facts of experience.

Now it is perfectly evident that this particular kind of tonal "relationship," arising out of certain acoustical properties of the sounds, is not the sole kind of relationship which may bind tones together in our experience. Two tones may come to be felt as related, in a way, merely because they have often been heard together. Moreover any two tones whatsoever, be their ratios simple or complex, are felt to be related to each other as higher and lower. Here the term relationship is used in its ordinary broad, untechnical sense.

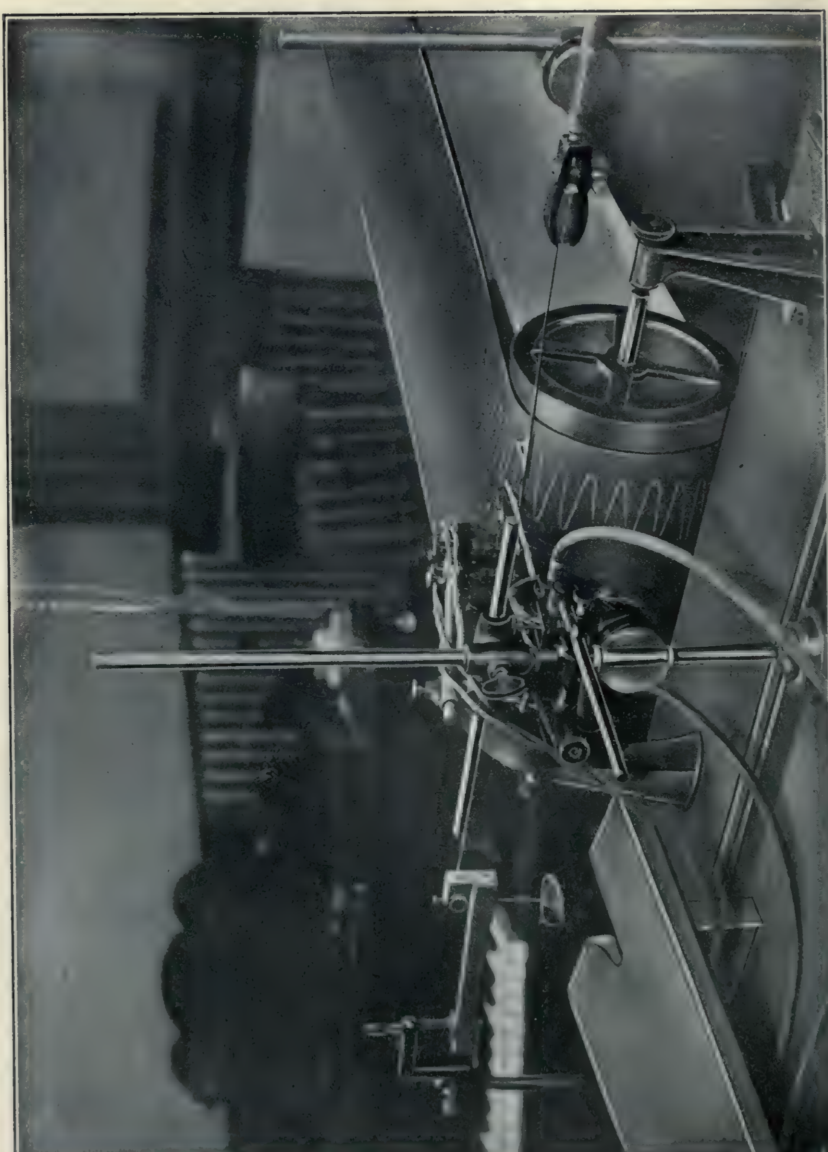
Whenever, in the following pages, the terms "relationship" and "related" are employed in the technical sense, they are enclosed in quotation marks; and where these marks are not used, the reader is to understand that the broader, untechnical connotation is indicated.

What the musician designates as tone-color or timbre, I have called by the usual psychological terms, clang-color, or briefly, color.

CONTENTS

Part I. The Melody Problem

Part II: The Phenomena of Melodic "Relationship," and of Melodic Trend.



PART I.

THE MELODY PROBLEM.

§1. Neither musicians nor psychologists are agreed as to the meaning of the term melody. Divergent usage, leading to misunderstanding and dispute, has arisen because within the range of melody experience there exist several distinguishable mental phenomena, each of which has in turn been construed as the essential mark of a melody. Weinmann,[1] following Lipps,[2] says that a melody is a *unity*, a whole, no mere succession of tones. It is, further, an esthetic unity in which the constituent tonal elements are subordinated to a single dominating element, the tonic. This definition operates to limit the scope of his study to such melody phenomena as those exhibited in modern European diatonic music, since it *a priori* excludes the possibility of melodies which lack tonality.

The doctrine of Lipps and his followers that esthetic unity always involves the *subordination* of the separate elements of a manifold to a single chief element is opposed by Meyer[3]. In his view, the statement that a melody is a unity means merely that we experience *relationship* between the tones. Indeed Meyer defines melody in terms of relationship.[4] To say that two tones are related and to say that they form a melody is the same thing. Such a definition avoids a narrow conception of melody. The scope of the term becomes much contracted, however, by the technical meaning which Meyer attaches to the term relationship. The essence of melody consists, for Meyer, not in the experience of any kind of relationship whatever between the successive tones, but in the experience of a very

[1] Fritz Weinmann: "Zur Struktur der Melodie" *Zeits. f. Psychol.* 1904, *35*, 340.

[2] Th. Lipps: "Zur Theorie der Melodie," *Zeits. f. Psychol.* 1902, *27*, 237. See also his *Psychologische Studien*, 2te Aufl. 1905, 193 ff.

[3] M. Meyer. "Unscientific Methods in Musical Esthetics." *Jour. of Phil. Psy., and S. M.* 1904, *1*, 711.

[4] Elements of a Psychological Theory of Melody. *Psych. Rev*, 1900, *7*, 246.

special and limited kind of relationship, namely that to which the technical musical term "relationship" has come to be applied. This narrowing of the meaning of the term operates to exclude from the realm of melody those songs of primitive peoples in which vague and indefinite pitch intervals appear, as well as the so-called melodies of speech.

Can we assent to Meyer's contention against Weinmann that melodic unity means nothing more than relationship between the parts? The esthetic unity which characterizes a melody does indeed involve experience of relationship among the several tones; but this is not all. For example, it involves also the experience of completeness. If the feeling of completeness is destroyed, the 'unity' is shattered. Not merely tonal relationship, but 'form' is necessary to constitute the esthetic unity of a melody. Meyer's deed here is better than his word: for throughout his investigations he searches for something more than mere "relationship" in his melodies, namely, for an *organization* of relationships, a combination of related tones ordered in one way rather than another,—arranged, indeed, so that they generate not a mere consciousness that the elements are related, but a perception that they are so related as to form a complete structure, a whole.

There are then, three clearly distinguishable phenomena, each one of which has been put forward as the peculiar differentia of melody: (*a*) "relationship" between the constituent tones; (*b*) esthetic unity or wholeness, such as distinguishes a definite melodic phrase when contrasted with a mere fragment of melody, or which characterizes even more clearly a complete melody that is brought into comparison with any portion of itself; (*c*) tonality, or the dominance of the entire sequence by a single tone, the tonic. Weinmann's definition stresses the third of these phenomena: if there exists a song of some alien people in which the preponderance of one tone over the others fails to appear, such a song must be called by some other name than melody. Meyer at the opposite extreme, emphasizes only the phenomenon of "relationship." Wherever "relationship" between successive tones is felt, a melody exists, even though the succession be fragmentary and the hearer be left in suspense, unsatisfied.

For the purpose of the present exposition, it has seemed best in defining what shall be meant by a melody, to place emphasis upon the second of these three phenomena,—upon the esthetic unity, the wholeness, which characterizes the completed experience. This usage of the term is adopted with full realization that it is not wholly unobjectionable. After such a definition, how shall one speak of Wagner's 'endless melodies?' By what name shall one describe the effect when in a Brahms chorus, one of the middle voices for a few brief measures stands prominently forth only to be lost to the ear again in a maze of counterpoint? Is not this tonal group without distinguishable beginning or end a most delightful melody? It would certainly be called a melody if, with Meyer, we had chosen to make "relationship" the sole essential; but in the terminology we have chosen, it must be called a melodic fragment, and not, strictly, a melody.

The matter of prime importance is, of course, to realize that by whatever names they may be called, we are confronted with three different phenomena—"relationship," phrase- or period-unity, tonality—which, no matter how intimately they may prove to be bound up together, are nevertheless in introspection clearly distinguishable, and must not be confused.

§2. At the risk of incurring the charge of prolixity from readers who are most at home in this field, I shall venture to develop somewhat more fully what I mean by a melody, before attempting to formulate explicitly the melody problem.

Let the reader ask himself in what way his experience of a melody differs from his experience of a mere succession of musical sounds of varying pitch. Possibly he will reply that the group of sounds that he calls a melody is more pleasing. But this agreeableness he will admit is not the essential character. One may, for example, upon hearing a flageolet of obnoxious tone quality find the whole experience disagreeable and yet recognize that what he is hearing is a melody; or on the other hand one may take delight in a perfectly random series of sounds drawn from a beautifully voiced instrument. Something other than the pleasurable affective aspect of the

total experience must be present to differentiate the melody from the non-melodic succession of pitches.

Upon further comparison of the two kinds of experience the observer will notice that the sounds of the melody seem to belong together, to cohere, and to stand in such a relationship each to the others that the entire series is felt to be a unity. The tones of the non-melody, by contrast, are felt to be unrelated: they do not 'hang together' as it were. Or, even if one discovers that some of the tones of the non-melodic group exhibit a close connection with some of the others, the group as a whole is not a unity: it is felt to lack consistency or internal coherence, or continuity, or completeness.

An example will make more obvious this contrast between the melody and the non-melody. I played to a group of moderately musical observers the following simple succession of musical sounds: $c' \; e' \; g' \; e' \; f' \; d' \; c'$. The tempo was slow, the duration of the tones uniform. I then played a second series beginning on the same tone and ending on the same tone, and employing the same five degrees of pitch as the first but in a different order: $c' \; f' \; d' \; g' \; e' \; f' \; c'$. The hearers reported that in the first group the sounds seemed to follow each other naturally, coherently, and in a way, inevitably, and with the last sound the series seemed to come to a definite close. Each element articulated with the others and the group as a whole was felt to be a unity. In other words, it was judged to be a melody. But with the second series of tones the hearers failed to discover this naturalness or inevitableness about the order of the sounds. The pitch, they said, wandered rather incoherently and disconnectedly here and there. Moreover when the last sound was heard it failed to bring the feeling of completeness, of finality, which characterized the close of the former series. This second succession of tones was judged by these observers to be no melody.

§3. Our definition of a melody places stress upon the experience of unity; but it does not prejudge the question as to whether this necessitates the subordination of all the elements to one dominating 'monarch element.' Neither does it imply that the experience of definite "melodic relationships" (in the technical sense of the term) is the *sine qua non*. A melody we

shall define as *a succession of musical sounds which is felt to constitute an esthetic unity*, a unity toward the establishment of which the *pitch* relations of the successive tones contribute.[1]

The melody problem, then, is the problem of explaining how a series of discrete tonal stimuli can arouse this feeling of unity.

As a matter of fact any actual melody such as a gamin whistles on the street or a Pawnee Indian sings to the dawn, gains its unity, its coherence, its wholeness, through the combined operation of many factors. The factors of intensity and duration, for example, are coördinate with pitch in the determination of the total psychosis: tempo, rhythm, dynamic structure share in determining what the melody shall be. A brief analysis of these factors will bring into prominence the particular phases of the melody problem with which these studies are concerned.

§4 It is to be remembered that musical sounds can vary one from another in only four ways: in duration, intensity, clang-color (i. e., tone-quality or timbre) and pitch. But each of these four aspects or attributes of the constituent tones affects in a two-fold manner the nature of the melody. The total effect is what it is, partly because of the *relative* duration, intensity, pitch and color of the separate sounds employed, and partly because of the *actual* pitch, intensity, duration and color. The 'actual duration' factor, for instance, is the tempo. The relative duration of all the sounds remaining constant, the nature of the melody may be entirely altered merely by changing the speed, i. e., the actual duration of the sounds. A familiar melody played in an unusual tempo may be hardly recognizable, and if the change of time is carried beyond certain limits in either direction the melody is utterly destroyed,—it becomes a confusion of noises or a broken succession of sounds without significance or interest.

Similarly, the actual or 'absolute' pitch of a melody enters in to make it what it is. The low rumbling melody with which Grieg begins the "Dance of the Trolls" in the first Peer Gynt suite is almost a totally different thing when played in the twice-accented octave, instead of three octaves lower.

[1] Here and throughout the paper, whenever the technical connotation of the term "relationship" is indicated, the word is enclosed in double quotation marks.

The difference which the actual clang-color makes is of course at the basis of artistic orchestration of melodies and of organ-registration. When a theme given out by the oboe is repeated by the violins we say it is the same melody, and yet it is not wholly the same.

Fourthly, the dynamic factor, the actual loudness or softness of the melody as a whole, remains to be mentioned as one of the contributors to the nature of the melody.

§5. These four factors taken in their actual or 'absolute' aspects are, however, of very secondary significance as compared with these same factors operating within the melody itself to contrast and to bind together the separate tonal elements. With reference to the *relative* duration, pitch, etc., of the individual tones, it will be convenient to treat of (*i*) the relation of each tone to its immediate associates, and (*ii*) the relation of the tone to the whole melody. (*Cf*. accompanying outline).

ELEMENTS OF MELODIC STRUCTURE CLASSIFIED ACCORDING TO THE FACTORS OF

I. DURATION	
a) Actual (*Tempo*)	*b*) Relative i. Measure pattern Rhythmical figuration ii. Accel., Rit., etc.
II. INTENSITY	
a) Actual	*b*) Relative i. Accent, stress, etc. ii. Cresc., decresc., etc.
III. COLOR	
a) Actual (*Orchestration*; *Registration*)	*b*) Relative i. ii.
IV. PITCH	
a) Actual (*Absolute pitch*)	*b*) Relative i. Interval relationships ii. Tonality relationships

Relations of duration of the first sort are at the basis of the measure-form and rhythmical figures, while accelerando and

ritardando illustrate the relations to a more inclusive group. Rhythm is usually a result of the combination of intensity and duration relations, although this is not always the case. Thus a melody played on the organ or on a mechanical piano player lacks variations of intensity of the separate tones.

In the case of the loudness factor, the former type of relation determines the effects of accent, of stress; while the latter gives dynamic form to the whole group, the crescendo-decrescendo effects, etc.

The relative color of the separate tones has, in the enumeration of the factors of melodic structure, usually been neglected. But *a priori*, one would expect this attribute of tone-sensation, as well as the others, to be of significance; and *a posteriori*, color is found to be of vastly greater importance to melody than one might suppose who had never given the matter careful thought. The reason why this factor has been overlooked is that it usually remains constant throughout the melody. Its presence as a unifying factor first comes into evidence when an unwonted change of color enters and makes itself felt as a disturbing element: as when a singer is not skillful in passing from one register of the voice to another, or a clarinetist meets a similar difficulty in making the transition from the lower to the middle register of his instrument. The changes in color which are thus unwittingly or unavoidably introduced have their disintegrating effect, be it never so slight, upon the melody. Among violinists this is a well known fact, a commonplace. Even so slight a change of color as is involved in the passage from one string to another is recognized as of importance in artistic phrasing, and the resources of technical proficiency are sometimes taxed in the effort to meet the requirements which this principle imposes. Such a principle raises a prohibition against careless shifts of color, and at the same time offers a positive aid to artistic phrasing,—it enables the violinist to give to a group of tones a peculiar unity of its own not otherwise obtainable. Surely such a factor in the determination of melodic form as clang-color,—a factor which has a recognized place in musical practice,—does not deserve to be entirely neglected. A careful experimental study of the effects and of the possible extent of alterations of color within the melody is a psychological desideratum.

§6. All of the factors which have been discussed, the relative clang-color, loudness and duration of the sounds, have been shown to contribute to the structural unity of a melody. But not all of these taken together are sufficient to make a melody. The essential factor is still lacking, namely the pitch relations. A sequence of tones of the proper relative loudness and duration to constitute a vigorous rhythm would not be called a melody if the pitch of the tones were either uniform or random.

The pitch, too, of each tone bears certain relations to the group of tones as a whole. This makes possible such phenomena as tonality, of which it will be necessary to treat in due time. At present let us focus attention upon the relations which may exist between individual tones.

These relations between tone and tone are of several distinct types. That type which has received fullest treatment at the hands of the musical theorist is the one which has appropriated to itself as a technical term the word "relationship." Two consecutive tones were said by Helmholtz[1] to be "directly related" if they form a perfectly consonant interval, in which case one of the clearly perceptible upper partials of the first is identical with one of the second; while to be "indirectly related" the two tones must each stand in some such direct "relationship" to a common third tone. This theory of "relationship" was used by him to account for the melodic intervals of the diatonic scale. To account for the appearance of chromatic intervals, 'accidentals', in melodies, Helmholtz further recognized a "relationship by propinquity"; the 'accidental,' he said, is 'related' to its neighbor by the mere fact of nearness. The fundamentally important type of "relationship" was, however, of the other sort; and since it had a basis in the physical laws of vibrating bodies, it naturally was described in terms of ratios of vibration rates. Like the phenomenon of consonance with which it is closely allied, direct "relationship" seemed to be dependent upon the partial identity of overtones which exists among "related" tones.

What now is the psychological phenomenon of which these physical facts seem to be the origin? In what way does one's

[1] H. Helmholtz, *Sensations of Tone*, tr. by Ellis, 1895, 256 and 350.

experience of a pair of "related" tones differ from that of a pair of "unrelated" tones? The difference is easily felt, but difficult to put into words. I shall here merely quote some more or less descriptive phrases from the records of my observers. When two "related" tones are heard in succession they are felt to 'cohere', to 'belong together', to 'articulate', to 'form parts of a larger whole.' "Unrelated" tones do not so behave. Rather they are felt to 'fall apart', to 'be unrelated'; 'they do not seem to belong to the same melody.' Tones at an interval of a major third exhibit a strong melodic "relationship." If the interval is increased by a quarter of a tone the "relationship" disappears. This phenomenon of "relationship" is not to be confused with that of consonance. The dissonant major second, for instance, is an interval whose tones exhibit melodic "relationship." What the significant connection is which exists between melodic "relationship" and consonance will be pointed out later.

Another type of relation which exists between the successive tones of a melodic interval may be called the relation of pitch distance. As regards their pitch all tones range themselves in a one-dimensional series, as higher or lower; and the relative position of two tones in this series finds its conscious representative in this feeling of pitch distance. Thus, the tone g' is felt to be at a certain pitch distance from c'; while its distance from d' is felt to be not so great. It is at once perceived that one's consciousness of the distance-relation between two tones is clearly distinguishable from one's consciousness of their consonance or of their "relationship."

It will be found useful to distinguish 'definite' from what may be called 'indefinite' pitch relations. The former are characteristic of all melodies which employ the definite intervals of a fixed scale. Some kind of 'indefinite' pitch relation must be experienced by that peculiar type of unmusical person who has no exact sense for intervals, but who enjoys hearing himself sing, and who can sing simple melodies in perfect time, and with so much sense for pitch relations as is shown in ascending when the melody should ascend, and then descending when the course of the melody takes a downward turn. The pitch-out-

line or melodic curve of his song corresponds in a vague, general way with the pitch-outline of the melody imitated, and in-so-far it betrays some kind of a sense for pitch relationship. These 'indefinite' pitch relations are characteristic of certain primitive melodies.[1] They also are of vast importance in the so-called melodies of speech. Indeed, the infinite variety of delicately expressive inflections which enrich our spoken intercourse must be recognized as based upon pitch relations of this 'indefinite' kind. The gross difference between the rising interrogative inflection and the falling assertatory is the most obvious example of this type of melodic relationship. The mental effects produced by mere rise in pitch have been described by Meyer in terms of effects upon the attention.

> A rise in pitch causes the hearer's attention to become strained, and the more so, the steeper the ascent, if I may use this expression. A fall in pitch, on the other hand, causes a relaxation of attention, a cessation of mental activity. . . . . The same strain and relaxation of attention is to be found in music. The normal end of a mental process is, of course, characterized not by strained, but by relaxed attention; for strained attention means continued mental activity. It is natural therefore that a melody ends with a falling inflection. . . . [2]

We shall have occasion frequently to refer to the significance for the melody problem of this "phenomenon of the falling inflection."

§7. If one carefully examines different melodic intervals to discover whether there may not be still other types of relation, he will probably disclose to himself a phenomenon which has received much attention at the hands of certain writers. He will notice that many melodic intervals exhibit a peculiar character which shows itself as a tendency for us to prefer one of the two tones as an end tone. The interval of the minor third, whose tones have the vibration ratio of 5:6, possesses no such attribute: one acquiesces indifferently in either the upper or the lower as a final tone. Neither tone has any very positive characteristics of finality about it. Not so, however, with the perfect fifth (2:3). If one hears it as an ascending interval, he is dissatis-

[1] *Cf.*, B. I. Gilman, "Hopi Songs," Jour. of Am. Ethnol. and Archeol. 1908, *5*, 14 and 224.

[2] *Am. Jour. Psych.*, 1903, *14*, 456.

fied, uneasy, and under more or less tension until he hears the first tone over again. But if it is a descending fifth which he hears there is acquiescence, satisfaction, repose, and no desire to hear the first tone a second time. One may say that one of these tones stands to the other in the relationship of 'tonic', or end-tone. This aspect of musical intervals will be called by the present writer their *melodic trend*.

Observation of this phenomenon as it shows itself in intervals of relatively simple vibration ratio has led some theorists, notably Lipps and his followers, to attach great importance to the 2 ratio. They find, for example, that the trend of the fourth (3:4) is very decidedly toward its upper tone as a final tone; of the major third (4:5), toward the lower; while the minor third (5:6) exhibits no noticeable trend whatever. The trend of the major second (8:9) is toward the lower, and of the minor second (15:16) toward the higher tone. Among the wider intervals, where the reader may perhaps feel that the phenomenon is not always so distinctly and unambiguously manifest, it is nevertheless held that the minor sixth (5:8) and the minor seventh (9:16) trend upward and the major seventh (8:15) downward, while the major sixth (3:5) shows no trend toward either upper or lower tone.[1]

It will be seen that in the case of every one of these 'pure' intervals the trend is toward that tone whose rate is a pure power of 2; 2 always becomes the tonic. Where neither rate is a pure power of 2, no trend is discovered. These phenomena have been grouped by Lipps under what he calls the 'law of the number 2.'

Kürzer gesagt:—Treffen Töne zusammen, die sich zueinander verhalten wie 2^n: 3, 5, 7 usw., so besteht eine natürliche Tendenz der letzteren zu den ersteren hin; es besteht eine Tendenz der inneren Bewegung, in den ersteren zur Ruhe zu kommen. Jene "suchen" diese als ihre natürliche Basis, als ihren natürlichen Schwerpunkt, als ihr natürliches Gravitationszentrum.

Dies ist naturgemäss um so mehr der Fall, je kleiner das (n) ist.

[1] These statements of typical trends are not completely in harmony with the results of the experiments described below. Differences are most in evidence in the case of the major and minor sevenths. See p. 25 *ff*.

(n) ist aber am kleinsten, wenn es gleich o ist. Und 2^{0} ist gleich 1. D. h. die vollkommenste Ruhelage und das letzte Gravitationszentrum solcher Töne bleibt immer der absolute Grundrhythmus.[1]

Upon this law of the compelling, dominating character of the 2 ratio, together with the principle that melodic "relationship" is closer the simpler the ratios, Lipps grounds his theory that a melody is a structure which gains its esthetic unity through the subordination of all its elements to one over-mastering ground-ratio, the tonic. This theory has been elaborated, in its application to modern European music, in admirable detail by Weinmann,[2] and defended vigorously by the author himself.[3]

In undertaking to explain *why* this phenomenon of melodic trend toward the power of 2 should manifest itself, Lipps makes one fundamental assumption, the assumption

that to the rhythm of the physical vibrations which generate a tone there corresponds an analogous rhythm in the accompanying processes of tone-sensation, or in the accompanying change of psychic or central conditions; that thus the psychic or central process of tone sensation is separated into a succession of elements or elementary partial processes analogous to the succession of physical partial processes, *i. e.*, to the single sound waves.[4]

Such a correspondence between the nature of central processes and the physical processes which arouse them, Lipps has found it necessary to postulate not merely in the realm of audition, but throughout the range of sensory experience. Esthetic pleasure results from inner harmony of our mental (or central) energies. A color-contrast is beautiful if there is a subconscious apprehension of the simplicity of the combination of the ether vibrations.

In the present state of total ignorance with reference to the intimate nature of central processes no attempt can be made

[1] Lipps. *Psychologische Studien*, 2 Aufl., 1905, 195. An identical formulation is given in his *Grundlegung der Aesthetik*, 1903, 465.

[2] F. Weinmann, "Zur Structur der Melodie. *Zeits. f. Psychol.*, 1904, *35*, 340-379 and 401-453.

[3] *Cf.*, especially, *Psychologische Studien*, 193 *ff*.

[4] *Zeits. f. Psychol.*, 1902, *27*, 228.

either to establish or to disprove such an assumption. By those who cannot follow Lipps in his bold hypothesis, his theory of the number 2 must be viewed merely as a description, not an explanation, of the facts.

Weinmann undertakes to buttress this theory of the basic nature of 'duality' in vibration-ratios by reminding the reader that 'double rhythm' is the original rhythm, the simplest, the most natural, etc.[1] But this is an argument from sheer analogy; for the experience of *rhythm* in the ordinary sense of the word has nothing whatever in common with the unperceived micro-rhythm of Lipps' assumption. One is a phenomenon open to introspection, observation and experimental study: the other is hidden, unknown, hypothetical.

Even though one may not relish such a theory as that of Lipps and Weinmann, and though one may be inclined to doubt the adequacy of their formulation of the facts by means of the law of the number 2, nevertheless the phenomena of melodic trend remain and must be reckoned with. Why is it that some melodic intervals seem to end better on the upper tone and others on the lower, while with still others it is a matter of indifference which of the two tones comes last? Why is a rising fourth more 'complete' than a rising fifth? Why does an ascending major second create a demand to hear the first tone over again, while an ascending minor second does not?

§8. No further attempt will here be made to enumerate with greater completeness the various mental phenomena which flow from the facts of pitch relationship. Only those have been mentioned which are of especial significance for these studies: pitch distance, definite melodic "relationship," indefinite pitch relations, consonance, melodic trend, the phenomenon of the falling inflection. We shall later have occasion to ask which of these phenomena are primary and which secondary or derived.

Our survey of the factors—of pitch, duration, clang-color and intensity relations—which contribute to the structure of a melody, makes possible a more definite formulation of the limited purpose of these studies.

[1] *Op. c.*, 342.

How the *pitch* relations of a series of discrete musical sounds may operate to weld these sounds into the organic whole which we perceive as a melody,—this is the core of the melody problem, and to this primary phase of the subject our present investigation will be strictly limited. To this end we shall consider pitch alone, and abstract as far as possible from all considerations of rhythmic figuration, accent, force, tempo, tone quality, etc., although these various factors would all demand attention in any account of the melody problem which aimed at completeness.

PART II.

THE PHENOMENA OF MELODIC "RELATIONSHIP" AND OF MELODIC TREND.

§9. The reports of previous experimentation specifically directed toward the melody problem are few in number.

One of the most original and suggestive workers has been Professor Meyer, and a survey of his contributions will serve to bring our own problem more clearly to view.

The first of Meyer's experimental investigations[1] led him to reject the theory of the diatonic scale, and to develop a new theory of melody. He used a reed organ specially constructed so that in playing a melody the performer was enabled, for each note of the printed score, to select any one of two or three tones of nearly the same pitch. Thus after repeated trials he could determine precisely what intonation of any particular melody was most satisfactory.[2]

Meyer published his analysis of some thirteen melodies, giving the intonation of each which seemed to him to be the best. These include melodies of folk songs and chorals as well as melodies from well known classical compositions. The reader is not surprised to find that the preferred intonation does not coincide with that of "equal temperament;" but neither does Meyer find that the melodies are most satisfactory when played in the justly intoned diatonic scale familiar to musical theorists. To be sure, in the simpler melodies, most of the pitches in the preferred intonation correspond exactly with the pitches when the melody is played in accordance with the diatonic scale. Some marked exceptions appear, however. Meyer finds, for instance, that

[1] M. Meyer: "Elements of a Psychological Theory of Melody." *Psych. Rev.*, 1900, *7*, 241–273. Reprinted with revisions and additions in "Contributions to a Psychological Theory of Music," *Univ. of Missouri Studies*, 1901, *1*, 1–80.

[2] A description of the instrument, with diagram of arrangement of keys on the manual is found in the *Zeits. f. Psychol.* 1903, *33*, 292.

the 'fourth' is preferred flatter and the 'sixth' sharper than diatonic intonation demands. To render the nature of these differences more clear, reference may be made to the accompanying table.

TABLE NO 1.

	c		*d*		*e*		*f*		*g*		*a*		*b*		*c*
Ratios of pitches in diatonic scale	1		9/8		5/4		4/3		3/2		5/3		15/8		2
		9/8		10/9		16/15		9/8		10/9		9/8		16/15	
	24		27		30		32		36		40		45		48
Some corresponding pitches from Meyer's Complete Scale.	1		9/8		5/4		**21/16**		3/2		**27/16**		15/8		2
		9/8		10/9		21/20		8/7		9/8		10/9		16/15	
	16		18		20		21		24		27		30		32
Diatonic scale	48		54		60		64		72		80		90		96
Meyer's	48		54		60		**63**		72		**81**		90		96

The first line of fractions shows the ratio between the vibration rate of each note of the diatonic scale and the vibration rate of the key note. Reducing these fractions to a common denominator, we obtain as the resulting numerators the numbers in the third row of the table. These are the numbers usually employed to express the relative pitch of the notes in the diatonic scale. (The ratio between the vibration rate of each note and that of the next note in the scale is given in the second line of fractions).

For comparison with these, I have selected from Meyer's 'Complete Scale' those notes which are used in the simpler melodies (see lines 4, 5 and 6 of the table).

It is to be noted, first, that the ratios in the diatonic scale involve no prime number but 2, 3, and 5, whereas the other scale employs the number 7 in its fourth. Thus, to tune *f* in the key of *c* one would not tune it a perfect fourth above *c*, but would tune it at an interval of an harmonic or sub-minor seventh (7:4) above the *g* below. Moreover the denominators of all eight ratios from the newer scale are pure powers of 2 whereas this is not the case with the fourth and sixth of the diatonic scale. The amount of difference in pitch which is involved is shown in the last two lines of the table where the ratios of the two scales are reduced to a common denominator for comparison.

To understand the significance which attaches to these differences, and other more marked differences in intonation which come to light in the more complex melodies, it is necessary to examine two "laws of melody" which, if one follows Meyer, lie at the basis of musical theory.

§10. The first of Meyer's laws of melody may be called the law of *melodic "relationship*:" Only tones which are "related," directly or indirectly, can belong to the same melody. The second, a law of melodic trend, is similar to Lipps' law of the number 2.

We will give Meyer's own formulation of what he means by the term "relationship."

When we hear successively two tones, the vibration rates of which are to each other as 2:3, or briefly speaking, the tones 2 and 3, we notice something not describable, which I shall call the *relationship* of these tones. To understand what is meant hereby, the reader may listen to the successive tones 7 and 11 or 11 and 10, in which cases he will notice that the two tones have no relation at all to each other.[1]

It is a fundamental contention with Meyer,—a contention that will demand our critical scrutiny,—that this psychological quality called "relationship" attaches only to pairs of tones whose ratios are expressible in simple fractions involving no prime number above 7.

That no relationship at all is to be observed with tones represented by the prime numbers 11, 13, 17, 19, etc., leads to the conclusion that only tones represented by the prime numbers 1, 2, 3, 5, 7, and their composites possess that psychological property.[2]

This leads to the theory of what Meyer names 'the complete scale.' Since none but related tones can belong to the same melody, and since "relationship" seems to exist only between tones represented by products of 2, 3, 5, and 7, the complete musical scale, or the series of all the tones which may occur in a single melody, is represented by the infinite series of all products of the powers of 2, 3, 5, and 7 (p. 249). The beginnings of such a scale, containing so many of these related products as were found

[1] Meyer: *Psych. Rev.*, 1900, 246.
[2] *Op. c.*, 247.

to be needed in the analysis of the melodies he studied, are given by Meyer in tabular form.

In maintaining that the 7 ratio exhibits the fundamental melodic qualities and must not be excluded from musical theory, Meyer takes sharp issue with traditional treatments of the subject. Lipps and his followers who have done more than anyone else to place the theory of melody on a basis of exact descriptive formulation find no need of ratios involving prime factors larger than 5. Other writers, as Helmholtz, Gurney and Stumpf have also been content with the theory of the diatonic scale, a scale whose ratios employ the numbers 2, 3, and 5, but not the number 7. Against these, Meyer brings the charge that they have been influenced primarily by considerations involving the phenomena of harmony, and have failed to point out what facts observable in *melody* justified them in excluding the number 7. The facts as he finds them are that such melodic intervals as the sub-minor seventh (4:7) the sub-minor fifth (5:7) the septimal second (7:8), etc., do possess the pyschological quality of "relationship;" and what is of more weight, he finds that melodies played in his so-called complete scale, which admits the 7 ratio, are preferred to the same melodies played according to the diatonic scale.

Meyer has been subjected to criticism for publishing his experiments and basing an elaborate theory upon them, when the judgments of preference recorded are apparently those of a single observer, namely, the author himself. Meyer admits the force of these criticisms, but insists that even so much of induction and carefully systematized observation as this report of his studies embodied, has more claim upon the attention of a scientific reader than all the great mass of writing upon musical theory which has no scientific, inductive basis whatever.

How does Meyer account for the phenomena of melodic "relationship?" How does he explain the fact that we feel the tones 2 and 3 to be "related" and the tones 11 and 10 "unrelated?" In contrast to Lipps he does not attempt to account for the facts. On the other hand he frankly admits that he is not offering an explanation of the melody phenomena: for this, as well as for an explanation of the facts of consonance we must

await further light upon the nature of neural activity and the action of the sense organs. All that Meyer is attempting, then, is to comprehensively *describe* the facts.

His first step toward this descriptive formulation has already been mentioned. As a result of his examination of the phenomenon of melodic "relationship" he decided that all cases of "relationship" are capable of being expressed in relatively simple fractions involving no prime factors except 2, 3, 5, and 7; and consequently the 'complete scale' is limited to tones expressed in these numbers and their compounds. The second step is the formulation of a law of melodic trend similar to, but not identical with, that of Lipps:

> When one of two related tones is a pure power of 2, we wish to have this tone at the end of our succession of related tones, our melody.[1]

Expanded to cover melodies of more than two tones, the law assumes the following form:

> No hearer is satisfied if after having heard once or more often the tonic 2 he does not find 2 finally at the end of the melody.[2]

In the elaboration of his theory Meyer utilizes two additional principles. One of these is that among "related" tones there exist different degrees of "relationship." The other principle is that of all those intervals which possess a certain "relationship" we have a decided preference for the smallest. The detailed development of the theory based upon these principles we shall not here undertake to summarize, but its foundations we must pause to examine more closely. It is obvious that there is need of conclusive evidence supporting the basic proposition upon which the theory is erected, the proposition that tones representable by the prime numbers up to and including 7 alone exhibit "relationship."

As evidence Meyer presents, as we have seen, two groups of facts, one derived from an examination of separate intervals and one from observation of the use in actual melodies of the 7 ratio. In both cases, as Wead[3] has pointed out in his penetrat-

[1] *University of Missouri Studies*, *I*, 9.

[2] *L. c.*, 24.

[3] C. K. Wead, *Psychological Review*, 1900, *7*, 400.

ing review of Meyer's work, the judgments recorded are apparently those of a single observer, and he a man of harmonic training. What indication is there that one who had never become familiar with anything comparable with our European harmonic musical system would experience these elementary "relationships?" "Nothing," says Wead, "can be more certain historically than that these relationships have been unrecognized by most of the men throughout the ages who have concerned themselves about music." One cannot avoid asking the question whether Meyer's deductions necessarily hold for hearers of melody other than those who, like himself, have long experienced the associations of modern European music.

A somewhat similar question arises regarding the effects of practice in detecting these melodic "relationships."

Meyer leads us to understand that only after long and careful observation did he decide that 5:7 and 7:8 exhibit "relationship." In another connection he proves[1] that "relationships" not detected at first come later to be felt, upon greater familiarity. This seems to place him in a dilemma. May it not be that the familiarity breeds the "relationship?"

It would not be rash to hazard that if Meyer had chanced to spend his early years in the Scottish Highlands it would never have occurred to him to exclude 11 while admitting 7 among the prime factors of his 'complete scale;' for in listening to the bagpipe he would have become accustomed to the interval 11:12,[2] would have learned to recognize it accurately, and to feel "relationship" between 11 and 12 as truly as between 15 and 16, or 7 and 8.

As long as the question remains unsettled regarding the inclusion or exclusion of 7, 11, or any other ratio in making up the list of elementary "relationships," a certain doubt will remain regarding the validity of Meyer's experiments on the intonation of actual melodies; for, in selecting the preferred pitches the observer's choice of alternatives for each note, it will be remembered, was limited to the two or three tones

[1] See below, p. 40.

[2] *Cf.*, A. J. Ellis: "On the Musical Scales of Various Nations," *Journal of the Society of Arts*, London, 1885, *33*, 499.

available from the scale constructed out of products of 2, 3, 5, and 7.

Instead of attempting here to settle this issue, let us ask some further questions with reference to Meyer's two main contentions. Is it true that only intervals the ratios of whose vibration rates are expressible in small prime numbers manifest the psychological quality of "relationship?" Is it a fact that of two "related" tones whose ratio can be thus expressed, the hearer always prefers as an end-tone that one which is a pure power of 2?

§11. First let us consider the fact of melodic "relationship." The major third is an interval which exhibits the character of "relationship" very unambiguously. This is an interval whose tones have the vibration ratio of 4:5. Now, what is the effect when we listen to an interval just barely wider than this, say the interval 400:501? It so happens that this interval exhibits the "relationship" more clearly, if anything, than 4:5 did,[1] although it is so nearly the same interval that those without special training cannot tell the two apart. Suppose this interval to be made a trifle larger yet, so that it has the ratio 400:504. Do the tones suddenly lose their character of "relationship?" One would hardly expect them to do so. Precisely what does occur is, that as the width of the interval is gradually increased it begins to change somewhat in character; but it remains a major third,—not a satisfactory third to be sure, but nevertheless a third with the *characteristic "relational" attributes* of that interval,—until it reaches nearly to the middle of the zone which divides the major third from a perfect fourth.

The experimental evidence, if any is required, in support of these statements, is easily obtained. The procedure adopted by the writer was to determine the effect produced upon the feeling of "relationship" by gradual but supra-liminal variations in the size of a melodic interval. Between the *b* and *c'* of a harmonium six reeds were interpolated, giving seven intervals, each of a magnitude of about 16 cents (*i. e.*, hundredths of

[1] Stumpf and Meyer found that all of the consonant intervals larger than a minor third are preferred too large. C. Stumpf and M. Meyer, "Maassbestimungen über die Reinheit consonanter Intervalle." *Zeits. f. Psychol.*, 1898, *18*, 321.

an equally tempered semi-tone). Such an interval in this region of the scale means a difference in pitch of scarcely more than two vibrations. It was thus possible to play any desired diatonic interval and also any one of half a dozen intervals intermediate in magnitude between it and the next larger interval. Only the major third and the fourth were tested. The method was without knowledge. The twelve observers were already familiar with the phenomena of "relationship" and finality in two-tone combinations. They were ignorant of the nature and purpose of the experiment. The observer was asked whether or not the two tones played were "related," and if the response was in the affirmative the further question was put, regarding the completeness or incompleteness of the two-tone group.

It was found, with each of the twelve observers, that the characteristic feeling of "relationship" was nearly always still present when the interval had been increased (or diminished) 32 cents, (a third of an equally tempered semi-tone). The characteristic feeling for the upper or the lower as an end-tone also remained. An alteration, however, of 48 cents (roughly a quarter of a tone) destroyed the feeling of "relationship" in 74 per cent of the 96 judgments.

In general, when a pure interval is gradually modified its characteristic melodic qualities remain long after the interval has lost the characteristic qualities, *e. g.* of consonance, which it manifests when its two tones are heard simultaneously instead of in succession. This fact ought to be of weight for any theory of melody which lays emphasis upon the psychological quality of felt "relationship." Since the ratio 3:4 has no monopoly upon the characteristic "relational" qualities of the fourth, but is rather only a modal ratio about which cluster an immense number of larger and smaller ratios manifesting in some measure identically the same psychological qualities, the use, without qualification, of the symbol 3:4 to represent that particular kind of "relationship" is misleading.

What is true in this respect regarding the facts of "relationship" is of course equally true regarding the facts of finality or melodic trend.

It may be urged that we are here confronted simply with the

common characteristic of perception, the modification of sensory data by central processes so that these data may be apperceived to the nearest available norm. Such tests as the above then would merely measure the tendency of the listener to hear different nearly equal intervals as the same pure interval, and do not prove that the "relationship" of the fourth inheres in any other ratio than 3:4.

But such a view neglects the fact that when we are listening to an interval slightly larger than 3:4, we may recognize it as larger and still at the same time experience the feeling of "relationship" characteristic of the fourth. The "relationship," in other words, inheres not merely in the interval 3:4, but also in intervals recognizably larger or smaller than the justly intoned perfect fourth.

§12. We shall not, however, press this consideration. Instead we shall leave in abeyance the question regarding the range of applicability of the pure powers of 2 formula, and shall seek, in the results of the experiments now to be described, the answers to certain questions with reference to the melodic trend in intervals with the simplest arithmetical ratios,—the intervals in which we are led to expect that the phenomena will be most in evidence. Does experiment establish the proposition that when one of two related tones is a pure power of 2, we wish to have this tone at the end, and that when neither of the related tones is a pure power of 2, no preference is felt for either as an end-tone? What is the relative strength of the trend in different two-tone combinations? Do the simplest ratios exhibit it most definitely? Do all observers feel it alike?

The method of the experiment was to present two tones in succession, and ask, "Can you make this second tone a final tone? Does this melody end?"[1]

The following series of ratios was used: 2:3, 5:6, 3:5, 15:16, 45:64, 4:5, 9:16, 32:45, 8:9, 8:15, 5:8, 3:4. This series was given in the 'double fatigue order,' both ascending and descending. Ten of the twelve ratios are relatively simple. Two, the aug-

[1] At the time when these experiments were planned, the experimenter was using the term 'melody' in the sense in which Meyer uses it. When the word implies nothing except "relationship," it is entirely appropriate to speak of melodies of only two tones

mented fourth and diminished fifth (32:45 and 45:64), involve pure powers of 2 but are not simple, and were included for purposes of comparison. Heavy Koenig forks mounted on resonance boxes and actuated by a rubber mallet were used as the source of sound. Each tone was sounded for five seconds. The range of pitch was limited to the once and twice accented octaves, the lowest fork being the middle c' of 256 d. v. and the highest the g'' of 768 d. v. In arranging the series *care was taken that neither of the tones of any pair belonged to a tonality which might have been suggested by the interval preceding.*

Eight persons served as observers in this series. None of them would be classed as totally unmusical, and none of them are "musicians," yet they represent, between these extremes, a wide range of musical ability. All are familar with musical notation and sing or play some from note. With at least two of the observers, there is a lack of interest in music, their skill at the piano being a mechanical acquisition. Three of the observers confessed to an acquaintance with the elements of harmony and musical theory, but it was evident upon trial that their theoretical knowledge was not concrete enough to exert any influence upon their immediate judgments of musical intervals. It may be remarked here that throughout these and also the later experiments the observers gave unreasoned judgments, the introspective records on this point confirming the opinion of the writer based upon the manner of their replies. All the observers had had training in experimental psychology.

The accompanying table gives the affirmative, doubtful and negative judgments of each of the eight observers with respect to each of the melodic intervals used.

TABLE NO. 2

Two Tones Heard in Succession. "Is the second tone a final tone?"

INTERVAL	OBSERVERS	An.	Td.	Bl.	Wl.	Rn.	Dg.	Mc.	Yo.	TOTAL
Minor Second, Ascending (15:16)	Affirmative	2	1	2	3	4	2	1	2	17
	Doubtful	1	0	1	0	0	0	1	1	4
	Negative	1	3	1	1	0	2	2	1	11
Descending	Affirmative	1	0	0	0	0	2	1	0	4
	Doubtful	0	0	2	0	0	0	1	1	4
	Negative	3	4	2	4	4	2	2	3	24
Major Second, Ascending (8:9)	Affirmative	1	0	2	0	1	0	2	0	6
	Doubtful	2	0	0	0	1	0	1	1	5
	Negative	1	4	2	4	2	4	1	3	21
Descending	Affirmative	2	2	2	3	2	3	2	4	20
	Doubtful	1	1	1	0	1	0	1	0	5
	Negative	1	1	1	1	1	1	1	0	7
Minor Third, Ascending (5:6)	Affirmative	2	0	2	0	1	0	2	0	7
	Doubtful	1	0	2	0	1	0	0	1	5
	Negative	1	4	0	4	2	4	2	3	20
Descending	Affirmative	3	0	2	1	0	1	1	2	10
	Doubtful	1	0	1	0	1	0	1	0	4
	Negative	0	4	1	3	3	3	2	2	18
Major Third, Ascending (4:5)	Affirmative	3	1	0	0	0	0	0	1	5
	Doubtful	0	1	3	0	0	0	1	2	7
	Negative	1	2	1	4	4	4	3	1	20
Descending	Affirmative	3	3	4	4	4	4	4	2	28
	Doubtful	1	1	0	0	0	0	0	1	3
	Negative	0	0	0	0	0	0	0	1	1
Perfect Fourth Ascending (3:4)	Affirmative	4	2	4	3	3	4	0	1	21
	Doubtful	0	1	0	1	1	0	1	2	6
	Negative	0	1	0	0	0	0	3	1	5
Descending	Affirmative	0	0	2	0	2	0	1	3	8
	Doubtful	1	2	2	2	1	1	2	0	11
	Negative	3	2	0	2	1	3	1	1	13
Augmented Fourth, Ascending (32:45)	Affirmative	0	2	2	1	1	0	0	2	8
	Doubtful	0	1	2	0	0	0	0	0	3
	Negative	4	1	0	3	3	4	4	2	21
Descending	Affirmative	0	0	2	0	0	1	2	1	6
	Doubtful	0	1	2	0	0	1	2	1	7
	Negative	4	3	0	4	4	2	0	2	19

Cont. of TABLE No. 2

INTERVAL	OBSERVERS	An.	Td.	Bl.	Wl.	Rn.	Dg.	Mc.	Yo.	TOTAL
Diminished Fifth, Ascending (45:64)	Affirmative	0	1	1	0	0	0	1	0	3
	Doubtful	0	2	2	0	0	0	0	1	5
	Negative	4	1	1	4	4	4	3	3	24
Descending	Affirmative	0	1	1	0	0	0	1	0	3
	Doubtful	0	1	2	0	1	0	1	0	5
	Negative	4	2	1	4	3	4	2	4	24
Perfect Fifth, Ascending (2:3)	Affirmative	1	0	2	0	2	1	1	3	10
	Doubtful	2	2	1	0	1	1	1	0	8
	Negative	1	2	1	4	1	2	2	1	14
Descending	Affirmative	4	4	4	4	4	1	1	4	26
	Doubtful	0	0	0	0	0	2	1	0	3
	Negative	0	0	0	0	0	1	2	0	3
Minor Sixth, Ascending (5:8)	Affirmative	1	4	4	2	1	0	1	1	14
	Doubtful	3	0	0	0	2	1	0	0	6
	Negative	0	0	0	2	1	3	3	3	12
Descending	Affirmative	0	3	2	0	1	0	0	2	8
	Doubtful	4	0	1	0	1	0	1	1	8
	Negative	0	1	1	4	2	4	3	1	16
Major Sixth, Ascending (3:5)	Affirmative	1	2	3	2	1	1	1	0	11
	Doubtful	1	0	0	1	2	0	1	0	5
	Negative	2	2	1	1	1	3	2	4	16
Descending	Affirmative	1	2	3	1	0	1	0	0	8
	Doubtful	2	0	1	1	3	0	2	0	9
	Negative	1	2	0	2	1	3	2	4	15
Minor Seventh, Ascending (9:16)	Affirmative	0	1	3	0	0	0	1	2	7
	Doubtful	1	3	1	0	0	0	1	0	6
	Negative	3	0	0	4	4	4	2	2	19
Descending	Affirmative	0	2	2	0	3	0	2	0	9
	Doubtful	2	0	2	0	0	0	1	0	5
	Negative	2	2	0	4	1	4	1	4	18
Major Seventh, Ascending (8:15)	Affirmative	0	2	2	1	0	0	2	2	9
	Doubtful	1	1	2	2	0	0	1	0	7
	Negative	3	1	0	1	4	4	1	2	16
Descending	Affirmative	1	0	2	2	1	0	2	0	8
	Doubtful	0	2	1	0	1	0	0	0	4
	Negative	3	2	1	2	2	4	2	4	20

§13. These results indicate that the descending major third (4:5) and the descending perfect fifth (2:3) exhibit more of the quality of finality than any of the other two-tone combinations. The one was judged definitely to end 28 times, and the other 26 times, out of a possible 32.

The other intervals showing more affirmative than negative judgments are the ascending perfect fourth (3:4) with 21 affirmative judgments; the descending major second (8:9) with 20; the ascending minor second (15:16) with 17; and the ascending minor sixth (5:8) with 14.

The diminished fifth (45:64)—both ascending and descending—and the descending minor second (15:16) each have the highest number of negative judgments—24. These are the intervals that most clearly lack finality. The ascending major second is next with 21 negative judgments, followed closely by the ascending and descending augmented fourth, minor third, minor seventh and major seventh, and the ascending major third. The percentage of negative judgments of the ascending perfect fifth and the descending perfect fourth is the smallest of any of the intervals judged not to end.

The ascending minor seventh (9:16) and the descending major seventh (8:15) are both judged to lack finality, contrary to the law of the number 2, although their inversions, the major and minor second, conform to the law. The ascending minor seventh has only 7 affirmative judgments as compared with 19 negative; and the descending major seventh has 8 affirmative and 20 negative judgments.

What is the reason for the large number of negative judgments on these larger intervals? One answer is, that the tones of these wider intervals sometimes failed to arouse any feeling of "relationship." "Those two tones do not belong in the same melody." "That second tone cannot be a final tone because it has no connection whatever with the first." "No! The tones aren't related." Such introspections were frequently given when the wider intervals were used. These not highly musical observers experienced a sufficiently strong and definite feeling of "relationship" in the case of such a small interval as 8:9, but found all "relationship" lacking in the inversion of that same interval, 9:16.

This means that in formulating the facts of *their* musical experience it would not be permissible to do as Meyer has done, and "omit the number 2 as a factor," or in other words to treat the trend and the "relationship" in any interval as identical with that of its inversion.

§14. Three-fourths of the 24 combinations are judged *not* to end more often than to end. The total number of judgments is distributed as follows:

		PER CENT
Affirmative	256	33
Doubtful	135	18
Negative	377	49

If we leave out of consideration the more complex intervals, the augmented fourth and the diminished fifth, the totals stand as follows:

		PER CENT
Affirmative	236	37
Doubtful	115	18
Negative	289	45

From these facts it would seem that in general it is somewhat harder to accept the second tone of a two-tone sequence as final than it is to judge it to be lacking in finality.

§15. Do the results of these experiments indicate that descending intervals as such tend to cause the feeling of finality? To answer this question the data of Table 2 may be redistributed so that the totals for ascending and descending intervals may be compared. Following are the totals for all the intervals represented by simple ratios involving a power of 2, then for the more complex intervals (augmented fourth and diminished fifth) and the intervals whose ratios though simple involve no power of 2, and finally for all twelve intervals combined.

Simple Ratios Involving a Power of 2:

	ASCENDING		DESCENDING	
	TOTAL	PER CENT	TOTAL	PER CENT
Affirmative	89	35	111	43
Doubtful	49	19	43	17
Negative	118	46	102	40

Complex Ratios, and Simple Ratios without a Power of 2:

	Ascending Total	Ascending Per cent	Descending Total	Descending Per cent
Affirmative	29	23	27	21
Doubtful	18	14	25	20
Negative	81	63	76	59

Totals for all Twelve Intervals:

	Ascending Total	Ascending Per cent	Descending Total	Descending Per cent
Affirmative	118	31	138	36
Doubtful	67	17	68	18
Negative	199	52	178	46

In each group, tones which are powers of 2 had the position of first tone exactly as many times as they had the position of final tone; consequently it will not be far wrong to assume that any effects due to the operation of the law of the powers of 2 are cancelled.

There is found, especially in the first of these three summaries, some preponderance in favor of the descending intervals as more definitely final and of the ascending intervals as lacking in finality.

This effect of the falling inflection has been made the object of experimental determination by Meyer.[1]

Three tones of a reed organ were played a few times in irregular succession, ending on one of them. Then they were played in a similar way, ending on another one; and lastly, ending on the third tone. This was repeated until each subject had made up his mind and written down which of these three endings was the most satisfactory to him. . . . .

Two classes of experiments must be distinguished: one in which there was no tonic effect among the three tones; and one in which there were tonic effects. In the former case the three tones were represented by the symbols 3, 5, and 7; in the latter, by 2, 3, and 9. [The tones *e*, *g*, and $7b^b$ stand in the ratio of 3:5:7; *c*, *g* and *d* would be represented in Meyer's symbolism by 2, 3, and 9.] . . . . The three tones of one experiment were always within a single octave. Each of the three tones, however, had an equal chance of exerting its influence, *i. e.*, of being the lowest of the three. (P. 458.)

Where there was no tonic effect, the lower tone, whichever

[1] *Amer. Jour. Psych.*, 1903, *14*, 456.

it happened to be, was preferred as an end tone, the totals being 5 choices for the higher, 8 for the middle, and 17, or 57 per cent of the total, for the lower tone. In the other series, one of the tones was a 'tonic.' When this tone was also the lowest tone it was preferred as the end-tone in 86 per cent of the judgments. When it was the middle tone it received 70 per cent of the choices; and when it was the upper tone only 7 per cent.

These are striking results and one wishes that these experiments had been carried farther. Brief as they are, however, they serve to emphasize that the effect of finality at the close of a melody may be due in part to the operation of other causes than the powers of 2 phenomenon.

It thus is obviously desirable, in discussing the meaning of our own results, to separate as far as this is possible the finality effect produced by the falling inflection from that which is due to the more definite pitch relations of the tones.

§16. We shall first bring together the totals for those simple intervals (Group S) whose ratios do not include a pure power of 2, *i. e.*, the minor third (5:6) and the major sixth (3:5). The second summary will include the complex intervals (Group C) involving powers of 2, *i. e.* the augmented fourth (32:45) and the diminished fifth (45:64). Then will come the eight remaining intervals, all expressible in simple ratios one of whose members is a pure power of 2. These latter it will be convenient to separate into those intervals in which the 2 tone is the higher (Group H), and those in which it is the lower (Group L).

Group S. Simple Ratios without a Power of 2:

INTERVAL	*5 : 6*	*3 : 5*	TOTAL	PER CENT
Ascending				
Affirmative	7	11	18	28
Doubtful	5	5	10	16
Negative	20	16	36	56
Descending				
Affirmative	10	8	18	28
Doubtful	4	9	13	20
Negative	18	15	33	52

Group C. Complex Ratios Involving a Power of 2:

INTERVAL	32 : 45	45 : 64	TOTAL	PER CENT
Ascending				
Affirmative	8	3	11	17
Doubtful	3	5	8	13
Negative	21	24	45	70
Descending				
Affirmative	6	3	9	14
Doubtful	7	5	12	19
Negative	19	24	43	67

Simple Ratios Involving a Power of 2:
Group H. (Higher tone a Power of 2.)

INTERVAL	15 : 16	3 : 4	5 : 8	9 : 16	TOTAL	PER CENT
Ascending						
Affirmative	17	21	14	7	59	46
Doubtful	4	6	6	6	22	17
Negative	11	5	12	19	47	37
Descending						
Affirmative	4	8	8	9	29	23
Doubtful	4	11	8	5	28	22
Negative	24	13	16	18	71	55

Group L. (Lower tone a Power of 2.)

INTERVAL	8 : 15	2 : 3	4 : 5	8 : 9	TOTAL	PER CENT
Ascending						
Affirmative	9	10	5	6	30	23
Doubtful	7	8	7	5	27	21
Negative	16	14	20	21	71	55
Descending						
Affirmative	8	26	28	20	82	64
Doubtful	4	3	3	5	15	12
Negative	20	3	1	7	31	24

According to the Lipps-Meyer formula, intervals of Group H should end better on the higher tone, and intervals of Group L on the lower. Consequently in Group H the finality effect due to the 2 ratio is opposed by the rising-inflection phenomenon, but in Group L the two forces work together.

Comparing the totals for all the intervals which according to the law of 2 should end, *i. e.*, the ascending intervals of Group H and the descending intervals of Group L, we find 59 affirmative and 47 negative judgments in the first case, as contrasted with 82 affirmative and 31 negative judgments when the effects of the two forces are cumulative. The

influence of the falling inflection increases the proportion of affirmative judgments very noticeably. Preference for the descending intervals as more definitely final does not, however, come to light in comparing the descending intervals of Group H with the ascending intervals of Group L—intervals which according to the Lipps-Meyer law lack finality. In both cases the negative judgments are more than double the affirmative in number, and the totals are almost exactly the same in the two groups.

It is instructive to combine the totals for the ascending intervals of Group H and the descending intervals of Group L, obtaining in this manner the totals for all judgments upon intervals which according to the formula of Lipps and Meyer ought to be judged to end. These may be compared with the judgments upon the same intervals played in the opposite direction, which according to this law are characterized by lack of finality:

End Tone a Power of 2:

	Total	Per cent
Affirmative	141	55
Doubtful	37	14
Negative	78	31

First Tone a Power of 2:

	Total	Per cent
Affirmative	59	23
Doubtful	55	22
Negative	142	55

§17. This last summary presents strong evidence of the operation of some such tendency as that to which the Lipps-Meyer law refers. When 2 is the end tone, the two-tone group is said by these observers to end in 55 per cent of the instances, and not to end in 31 per cent, the remaining 14 per cent being 'doubtful.' When 2 is the first tone of the pair, the proportions are reversed. Only 23 per cent are judged to end, while 55 per cent are judged to be lacking in finality.

In attempting to account for the judgments which do not conform to the law, it is to be remembered that in exactly one half of the instances in each group the effect of the rising

or falling inflection was acting in opposition to the phenomenon under discussion. Hence a certain ambiguity and uncertainty is sometimes inevitable. But the inadequacy of this explanation to account for all of the facts becomes manifest, when we examine afresh the separate data from which these totals are compiled (p. 25). Why does the same observer declare at one time that the ascending minor third, for instance, ends, while at another time he declares with no less positiveness that it does not end? The fact that some of the observers were but slightly musical accounts for part of these anomalies,[1] but some contradictory judgments occur in all the records including those of the most musical observers. How can the latter be explained?

The suggestion was made that the fork tones were so nearly pure that the feelings of "relationship" were weak and consequently the reactions produced were not normal. But the real difficulty did not consist in any *lack* of feelings of "relationship" and of finality, but rather in the fact that these feelings were apparently often *misplaced*. Moreover, control tests with harmonium and piano tones rich in upper partials failed to decrease the proportion of contradictory judgments.

§18. To gather further data another series was arranged containing, besides the twelve of the original series, five additional intervals: 24:25, 9:10, 27:32, 20:27 and 27:40. Five quite musical observers served, including the two most musical of those who had assisted in the previous experiment. The procedure was varied by putting the question differently: "Do you feel any desire to return to the first tone?"

With the attention thus directed, it is not surprising that some of the observers reported with certain intervals that they desired to hear the first tone again, whichever way the melodies were played, ascending or descending. Thus was forced into notice what has been called the *law of the Return*, the law that, *other things being equal, it is better to return to any*

[1] For example, when observer Bl. reported that an augmented fourth ended satisfactorily on the upper tone, he was asked to hum the interval upon which he had passed judgment, and sang a perfect fourth. The same thing occurred in the case of Td, who, however, discovered after he had sung the interval that it was not the same as the one he had originally heard, and wanted to change his judgment upon it.

starting point whatsoever than not to return—a simple, fundamental principle of musical form, of art form of any kind, indeed.

Another law to which the introspections pointed is not so simply formulated. It was brought to attention by three observers who persistently found an additional alternative in the case of certain intervals: the melody lacked finality, there was no desire to return, neither tone would serve as an end-tone but some *third* tone was demanded. Here was a melodic trend, definite, positive, insistent; a property of a single pair of successive tones, but leading beyond them to something further.[1]

It was plain that the facts of elementary melodic "relationship" and the law of finality of two-tone melodies did not tell the whole story. The phenomenon of melodic trend seemed to be of a more complex sort, even in two-tone groups, than is implied by any statement of a tendency to return or not to return. Even with these simple two-tone sequences it was necessary to recognize the operation of some such law as the following: *Two melodically "related" tones tend to establish a tonality*, and the melody is judged to end only when the final tone is one of the members of the tonic triad—preferably the tonic itself.

This law is not asserted to be a universal law. Indeed it is doubtless limited in its application to the experience of those reared in a harmonic musical atmosphere. In so far as it is found to be valid, it indicates the probability that the phenomena of melodic trend are not primary, but are derived from our experience of consonance.

These experiments were supplemented by briefer and less systematic tests upon a number of observers, unpracticed in psychological observation. The results were in general confirmatory, although not as strikingly uniform as those we have already given. Mention will be made only of four of the observers whose records are exceptional. Two of these exhibited a persistent preference for endings that suggested

[1] These introspections complicated the records so much that it is not deemed advisable to reproduce them here in full.

the minor mode. Tested upon the interval of the minor third (5:6)—no tonality having been previously supplied—these observers uniformly judged the ending on the lower tone, (5), to be satisfactory, while the ascending interval was judged to be lacking in finality. One of these observers is a very musical Welshman, and it is to be recalled that much of the characteristic Welsh music is in the minor or as they call it, the "la" mode. Tests were made upon two Japanese young men who had recently arrived in this country and who professed to have had but little opportunity to hear European music. Both were singers and one was a performer upon the Japanese flute. The tests, repeated, gave very conflicting results, and it became evident that either the interpreter had failed to make clear to them precisely what the phenomenon was upon which they were to pass judgment, or else their experience of melodic trends differs essentially from ours. Unfortunately it was not possible to carry out an extensive series of tests with these observers.

§19. For purposes of comparison, a third set of experiments was undertaken in which the tonality feeling was not left to be contributed by the hearer, but was definitely suggested to him. In the previous experiments, the utmost pains had been taken to exclude the operation of tonality by arranging that neither of the tones of a given group should belong to any tonality which might have been suggested by the immediately preceding experiment. If any tonality was present, it had a subjective origin. We have seen that many apparently contradictory judgments were given, as for instance when a minor second was judged to end, now on the higher and at another time on the lower tone, both judgments being positive and emphatic.

In the experiments now under discussion, on the other hand, the device was used of controlling the tonality, imposing it from without and testing after the judgment had been made to see whether or not the objectively given tonality had been retained. To facilitate this procedure, a piano tuned in equal temperament was used instead of the forks.

These experiments were carried out upon five musical observers, practiced in psychological observation. Three of these were quite naïve as to the nature or course of the experiment.

All the intervals of the tempered scale exclusive of the octave were employed. Each interval was used, beginning at every possible position in the scale: thus the ascending fourth was heard, beginning on 1, 2, 3, 5, 6 and 7 of the scale. The series was given in double fatigue order. The experimenter noted down the observer's introspections regarding the trend of the interval, or trends, for several optional directions of melodic movement were often detected. In these instances where more than one leading presented itself to the observer, an effort was made to determine the relative strength of each.

The result suggested by the previous experiments came clearly to view: so long as the given tonality was maintained, the trend of any interval, ascending or descending, was toward some member of the tonic chord, preferably the tonic itself. Individual differences showed themselves as stronger or weaker demands for the tonic as the end-tone, as over against the third or fifth when the latter were nearer than the tonic. For example, in the key of *c*, observer Rn felt that the sequence g' f' demanded c' as its third tone, whereas the other four observers found the trend to e' stronger. The uniform tendency for all five observers, however, with all the intervals, was to rest in one of the tones of the tonic chord.

Our contention is that in the previous experiments with no objectively supplied tonality, the anomalous results and contradictions above mentioned are explicable on the hypothesis that tonalities, now one and now another, arose in the mind of the observer. The minor second e'-f' would at one time chance to suggest the tonality of *f* and end satisfactorily on the upper of the two tones; while at another time the tonality of *c* would arise, entailing quite different demands.

§20. We have too long neglected to specify what is implied, psychologically, in the term tonality. By a tonality is meant a group of mutually related tones, organized about a

single tone, the tonic, as the center of relations. Subjectively a tonality is a set of expectations, a group of melodic possibilities within which the course of the successive tones must find its way, or suffer the penalty of not meeting these expectations or demands of the hearer and so of being rejected as no melody. Of these different demands, that for an end on a certain tone is the strongest and most characteristic.

It is not meant to imply that this tonality, this system of related pitches with a common center of reference, is present in consciousness as a group of auditory images. Often there is only a single simple auditory or vocal-motor image or percept to be detected. The tonality consists in the *attitude* of which the image is merely the superficial manifestation or sensory core. One can image the tone of 320 d.v. as a tonic in the key of *e* or as a median in the key of *c*, and the auditory image will be identical in the two cases, but not the total psychosis. There will be an entirely different organization of expectations, an entirely different attitude, an entirely different set of anticipations and demands, a preparedness for one set of experiences, but not for another.

So much an impartial introspection cannot fail to disclose. The position here advanced is that these same "attitudes" are constituted in large part of kinæsthetic elements—reports of processes of *motor adjustment.*

Suggestions toward such an interpretation of the tonality phenomenon were abundant enough from some of the observers. When Ha. felt a melodic trend unrealized, he often described it as a *vocal tension*, due to a tendency to sing the desired pitch. An. reported kinæsthetic sensations from the throat as accompanying the feeling of expectation. He also mentioned sensations of strain and tension in other regions, notably the diaphragm, these general tensions being especially prominent at the instant when he was attempting to retain an elusive tonality against an auditory distraction (as when, for instance, given the tonality of c, he was asked to listen to the interval c-f.) Do. found that "the effort to hold a tonality involves general organic tensions. Any lapse

of attention or shifting of muscular tensions precipitates a shift of tonality. Changes of breathing will do this,'' etc., etc.

Considerations such as these pointed toward the value of an approach to the problems of the melody experience from the side of its motor accompaniments, and resulted in the experiments reported in Part III upon the motor effects of simple melodic stimuli.

Whatever the nature of a tonality 'attitude,' whatever its relations to sensations of strain and muscular movement—it is at least a phenomenon which widely pervades the musical experience of hearers who are familiar with European music. The question now arises whether either the tonality experience or the experience of finality in two-tone sequences is primary, original, fundamental: Does the law of 2 describe a primitive, natural tendency or preference, which has operated in the course of historical development to mould our musical system, or does it describe certain secondary, derived phenomena which would not be discoverable in an experience wholly uninfluenced by association? Proofs of the former alternative the writer has been unable to discover. Moreover, the history of our musical system points toward a gradual evolutionary process in which the *primary phenomena of consonance* have been efficacious factors. Hearers whose minds have been influenced by association with such a musical system, when listening to certain two-tone sequences cannot avoid feeling a preference for one of the tones as an end-tone. Some of these preferences lend themselves to formulation in terms of the Lipps-Meyer law of the number 2; but this law is only a *special case* of the more general law that every melodic interval trends toward one of the tones of the tonic chord of the tonality which it arouses. The law is based upon the tendency of every interval, yes, of even a single musical sound, to establish a tonality attitude. The manner in which the law operates will be evident from one or two simple illustrations.

What shall be said, for instance, of those curious, sometimes baffling experiences, in which a second tone is at first

unwelcome, and then quickly makes itself at home and usurps the place of what had before been anticipated as the final tone? In certain instances nothing is more natural or inevitable. The first tone arouses a slight tonality feeling, making itself the tonic, so that if we call this tone *c*, we shall have an 'attitude' in which any of the tones *c*, *e*, and *g* of the tonic chord (but especially *c* itself), would be welcomed as possessing something of the quality of finality. Suppose now we hear the rising fourth *c-f*. When *f* first enters, as a final tone it is not welcomed: it does not meet the requirements of those expectations aroused by the first tone. But *c-f* is a harmonious interval: it immediately tends to shift the organization of the tonality feeling to something which will *include both c and f* in one common tonic chord. This is, of course, the chord *f-a-c-f′*, of which *f* is the fundamental. If this transition is successfully made,— and the chances are that such a transition can be avoided only with conscious effort,—then *f* becomes a final tone, and the interval which at first felt incomplete and unsatisfactory comes to a definite close.

Why does the descending fifth end while the rising fifth does not? When one hears a tone *c* and then its fifth, both fit without readjustment into the *c-e-g* tonality suggested by the first tone, and for complete finality one wishes to hear again the tonic *c*. But if, instead of ascending from the tone *c*, we hear a descending fifth from the same starting point the situation is altered. The chord which includes the original *c* and this new tone *F* is the chord *F-A-c*. Our demand is, accordingly, to hear as a final tone the tonic of this chord, which is *F*. A similar treatment applies to every instance of "direct relationship" in which the law of 2 was found to hold good. This law of the powers of 2 is no primitive universal law: the phenomenon it describes is peculiar to those minds habituated to a musical system whose scale has a basis in the laws of consonance and dissonance.

§21. The overshadowing rôle played by habit or association in the drama of our esthetic experience is not always recognized. The effect of habituation in rendering disagreeable

sequences tolerable or pleasant and in changing unrelated into related tones, has been shown by Emerson[1] and also by Meyer[2], although the latter finds in his results substantiation for a very different contention, namely, the universal applicability of the "complete scale."

Emerson worked with extremely small melodic intervals and found that after much experience with these small intervals his observers developed preferences for certain sequences, showing that a melody can be constructed of tones all of which are within the compass of a semi-tone.

Meyer constructed some 'quarter-tone melodies' from the intervals of his complete scale. At the initial performance, the effect was judged by most of his observers to be disagreeable, but on repetition this judgment was modified, and two weeks later, at still another hearing, some of them came to appreciate and enjoy the music which had before been strange and incomprehensible. What an excellent illustration of the law that we do not accept as melodically good that which we cannot in some measure anticipate!

Subjected to careful introspective analysis, the feeling of finality attaching to the second tone in the interval 3:4 differs in no essential from the feeling of finality attaching to the last tone of a purely arbitrary tone combination with which one has grown familiar. In each instance the sense of finality consists of the same kinaesthetic sensations in throat and diaphragm, the same feelings of relaxation, the same repose, the same slight retardation in the rate of mental flow.

This effect of habituation is a familiar fact in the musical experience of everyone. Tonal sequences at first bizarre, strange, unmusical, later come to be appreciated, understood and enjoyed. Some degree of habituation to any succession of intervals whatsoever makes possible the act of recognition, of acknowledgment, of 'welcoming' the successive tones, to use Professor Royce's apt phrase. Habituation, then, is

[1] L. E. Emerson, "The Feeling Value of Unmusical Tone Intervals," *Harvard Psychological Studies*. 1906, *2*, 269.

[2] M. Meyer, "Experimental Studies in the Psychology of Music," *Am. J. Psy.*, 1903, *14*, 456.

sometimes a powerful factor in making possible that active participation which seems to be demanded of the hearer before the succession of musical sounds can for him be unified into the organic whole we call a melody.

§22. *Summary.* These studies began with a definition of melody which laid stress upon the feeling of unity. When the separate tones of a series are felt to be related to each other in such a manner that each tone forms part of a coherent whole, the succession of tones, we said, is felt to be a melody, and the melody problem was stated to be the problem of explaining how this feeling of melodic unity arises. An analysis of the psychological elements of melodic structure revealed many and varied sources contributing to the generation of this unity. One group of factors, however, stood out as of unique importance, namely those due to the relative pitch of the constituent tones; and to the consideration of problems in pitch relationships the scope of the present investigation was limited.

A survey of the efforts that have been made to reduce the facts of melodic "relationship" and of melodic trend to simple mathematical formulation was followed by an account of three sets of experiments upon the phenomena of melodic trend in two-tone groups. These trends, with which the feelings of finality or of lack of finality are closely bound up, were found to be due to (*a*) preference for the lower tone as such as an end tone (phenomenon of the falling inflection), (*b*) preference for a return to the first tone as an end tone, (*c*) preference for the expected ending (if one knows that a given tone is to be the last, its arrival may be sufficient to arouse the feeling of finality quite apart from the operation of any other factors), and, finally, (*d*) preference for an end on one of the tones of the tonic chord—and especially the tonic itself—of the suggested tonality.

This formulation, contrasted with the formulation in terms of 'the law of the number 2,' has the advantage of covering more of the observed facts[1] and the disadvantage, as some will consider it, of conceding that the phenomenon described is

[1] For example, the numerous instances in which 8 : 9 and 15 : 16 are judged to end better on the tone which is not a power of 2.

probably not elemental, primitive, but rather a resultant, traceable to the laws of habit and the harmonic structure of the music with which the observers were acquainted. According to this view, the laws of consonance are primary, not the laws of melodic "relationship."

This latter view finds confirmation in the instances cited where the feelings of "relationship" and of trend were clearly the outgrowth of habituation, of repetition, of custom, of association, of mere expectation.

Mention was made of the high importance which seemed to attach, in the introspections of certain of the observers, to kinaesthetic factors present in their experiences of tonality, "relationship" and trend. These facts, together with the fact that the phenomena of "relationship" are exhibited by pairs of tones which vary so widely from the simple ratios, suggest that it is not the sensory but the motor phase of the circuit which contributes the unity,—that it is not the relatively economical activity of the sensory nerves, but the relatively unified response of the motor mechanism which gives rise to the feeling of "relationship."

Our problem, then, shapes itself as the task of studying the motor responses which melodic stimuli elicit, to discover whether here is to be found any further clue to the explanation of melodic unity.

PART III

EFFECTS OF MELODIC STIMULI UPON MUSCULAR MOVEMENT

§23 To gather definite data regarding the relation of movement to the melody experience, the following experiments were undertaken, designed to test the effects of simple melodic stimuli upon on-going motor processes, voluntary and involuntary.

The voluntary process studied was the tapping movement of the index finger of the right hand. This movement was chosen because of its simplicity and naturalness, and because after a little practice it tends toward automatism, leaving the attention free to be focussed upon the stimulus. Such devices as the Jastrow automatograph and the Delabarre muscle-recorder were rejected in favor of the means here described, because it seemed highly probable that changes in innervation would become most readily manifest as alterations of a motor process already going forward. Other factors remaining constant, it is to be expected that a neural current will tend, at least in part, to find its way out of the central system along that motor channel which is already in use. Moreover the investigations of Stetson[1] and others upon complex or "combined" rhythms have made it certain that a concurrent movement coming into coördination will affect an accompanying uniform movement.

The form of apparatus used is an adaptation of the simple device employed by Stetson for recording rhythmical movements. The hand and forearm rested naturally upon the arm-rest leaving the index finger free to move throughout its entire range of flexion and extension without contact. (See accompanying figure). This free, unrestricted movement was chosen because it was found that when the finger taps against a hard surface the contact sensations serve as a

[1] R. H. Stetson: "A Motor Theory of Rhythm and Discrete Succession." *Psych. Rev.* 1905, *12*, 250.

sensory control which regulates and steadies the movement. As our purpose was to detect any slight variations which the melodic stimuli might produce in this motor process, it was obviously better to avoid as many of these controls as possible.

The periodic movement of the finger was recorded in all its details as far as changes in rate, form, and amplitude of movement in a vertical direction are concerned, by means of the recording device above mentioned. From the leather finger-cot a silk thread ran over a tiny pulley and through

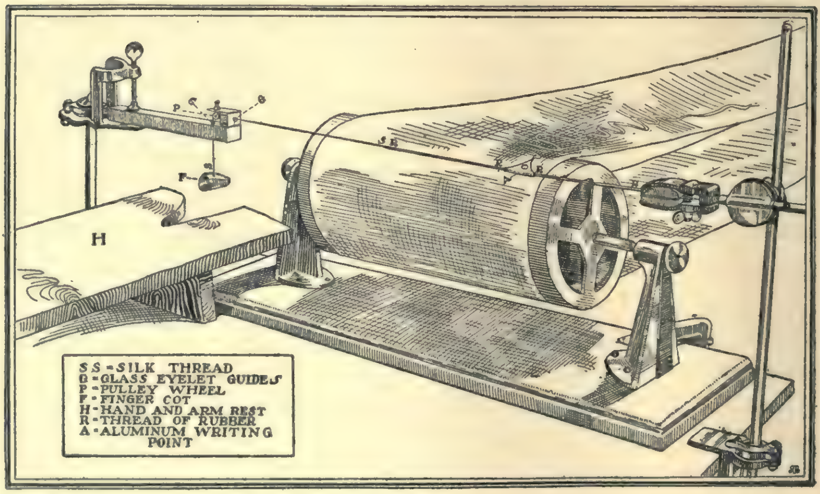


FIGURE No. 1

glass guides which prevented any loose motion. This thread led to a rubber thread, in the middle of which was an aluminum writing point, which traced a record of the finger movement upon the belt of smoked paper. A slight torsion of the rubber served to keep the writing point against the surface of the kymographic belt. By varying the length of the rubber on either side of the writing point the relative amplitude of the curve could be made as small as desired. Most of the records, however, were taken with all of the rubber upon one side of the writing point so that the curve was equal not only in form but also in amplitude to the vertical

component of the finger movement. The tension of this delicate rubber was so slight that it was barely perceptible to the observer, and did not interfere with the freedom and naturalness of the movement. Indeed, the superiority of this recording device over that of a tambour lever lies in the perfect freedom of lateral motion allowed; because there is no restraint upon the finger movement, there are no sensory controls other than those cutaneous and kinæsthetic sensations due to the movement itself.

The belt of smoked paper ran between two cylinders placed about two meters apart. It was driven by an alternating current, constant-speed motor whose only variations were due to fluctuations in the rate of the generator of the Cambridge lighting plant. Tests with vibrating forks of 50 d. v. and 500 d. v. showed that the maximum variations in the rate of the belt of smoked paper were less than one and one-half per cent. As a precautionary measure, however, a time line was made a feature of all the records, interruptions at periods of one second being furnished by means of a Lough self-actuating pendulum, placed in a distant room.[1] Precaution was taken to banish all sound which might arise from the recording apparatus, such as the ticking of the electric markers. The driving mechanism was placed outside of the experimenting room, as otherwise a low hum from the motor could be heard even when it was encased in a "sound-proof" box.

One electric marker, as has been said, furnished the time line. This line also served as base line for measuring amplitudes. Another marker was in circuit with the keyboard of the harmonium which was used for giving the melodic stimuli, and furnished the record of the course of the experiment.[2] A silent pendulum was used to aid the experimenter in controlling the length of the sounds. The smoked record was

[1] Only alternate taps of the time-marker, i. e., one every two seconds, are visible in the sample records reproduced on p. 51.

[2] It is the opinion of the experimenter that a simple pneumatic attachment to the keyboard of an organ or piano with tambour recorder would on the whole prove more satisfactory than an electrical attachment.

made permanent by being sprayed with a ten per cent solution of gum sandarac in alcohol.[1]

Pneumographs of the Sumner pattern were employed to record the abdominal and thoracic breathing. The degree of sensitivity of pneumographs and tambours is shown by the clearness with which the pulse-beat appears on the pneumographic tracings, quite plainly enough indeed, especially on the curve of the abdominal breathing, to permit the computation of the pulse rate if desired.

Nothing of significance for the present investigation appeared, however, in these pneumographic curves. The reason doubtless is found not in the fact that melodic stimuli do not produce important modifications in the breathing, but rather in the fact that the duration of the stimulus used was too brief to permit the characteristic alterations to appear. In this respect the conditions were quite the reverse of those in the experiments of Foster and Gamble.[2] These experimenters using musical selections of various kinds as stimuli found that listening always tends to shorten the expiratory pause and to make the breathing faster and shallower, but not steadier; but no remarkable differences were found in the effects of loud and soft or major and minor music. One is not surprised to learn that characteristic breathing phenomena could not be isolated when use was made of such highly complex stimuli as actual musical selections.

§24 When the subject had taken his place and the pneumographs and finger apparatus had been adjusted, the nature of the particular experiment to be performed was explained. The number of tones which were to be used was told, but nothing further was said regarding the nature of the melodic intervals. The subject then closed his eyes and the experimenter started the kymograph, so that a brief record of the breathing was obtained before the finger movement began.

[1] The double-glazed paper used was too thick to be fixed by the usual device of painting on the wrong side. The use of a spray proved to be convenient and expeditious. A "fixative spray," to be had for ten cents at any art store, when fitted to a foot-power bellows, proves very satisfactory.

[2] Eugenia Foster and E. A. McC. Gamble: "The Effect of Music on Thoracic Breathing." *Amer. Jour. Psych.*, 1906. *17*, 406.

At a word from the experimenter the subject began the tapping movement taking whatever rate was most natural to him. After the tapping had continued for twelve seconds or longer the melodic stimulus was given. The tones were played upon a reed organ the mechanism of which was in electrical connection with a marker which recorded the instant of depressing and raising the keys. The general plan was for the experimenter to sound each tone for a period of three seconds. It may be thought that this period was unnecessarily long, but the observers did not find it objectionable and it has two very obvious advantages. In the first place a period as long as three seconds is sufficient to permit any motor changes which the stimulus may produce to become evident in the record of the finger movement. And in the second place the use of the three-second period minimized, if it did not indeed entirely rule out, the factor of rhythm. Stimuli whose rate is as slow as one in three seconds do not tend to become rhythmized.

After the melodic stimulus the tapping was continued for ten seconds or longer. The observer was then called upon to give his introspection. Aside from a general introspective record of the course of the experiment, the naturalness of the tapping, effect of external disturbances, and the like, the points toward which inquiry was especially directed were two: first, does this melody end? Has it the characteristic of finality, or is it unfinished? Does it leave you in suspense? Does it demand something further? Secondly, the question was raised as to whether or not the melody was pleasing. In many cases but not in all, these two aspects, the affective and the aspect of completeness, seemed to be felt as identical; that is to say, a melody was judged to be agreeable because it came to a good ending, or to be unsatisfactory because incomplete. Not infrequently, however, one met with introspective reports like the following: "That is good; I like that but it is not finished," or, " That isn't particularly pleasant, but it ends very emphatically."

A word ought to be said about the way in which the observers were first brought to an understanding of the phenomenon

which was under investigation. They were not told what the phenomenon was, and then asked if they could observe it. On the contrary, the plan employed was to play an interval of an ascending fifth and then to play the same interval descending and then ask for a full introspective account. Some observers would quickly detect the feeling of relaxation, of repose, of completeness which accompanied the perceptiom of the descending fifth and which was lacking when the ascending fifth was heard. Lest they should immediately form the opinion that this characteristic of finality always accompanied a descending interval, the perfect fourth was next played. This interval they soon discovered makes a better ending upon the upper tone than upon the lower. Only after the observers had become thoroughly familiar with the phenomenon were they asked to serve as reagents in the main experiments. With two of the observers not a little persistence together with many repetitions of the intervals was required before they discovered the phenomenon, but in every case it was a genuine discovery of their own, and was not suggested to them.

§25 The observers were research students or instructors in the Harvard Psychological Laboratory, with the exception of Po., who had, however, had training as an observer elsewhere. All with the exception of Da. and Pu. were men.

It will be convenient to divide the observers into three groups according to musical ability. This classification is based upon tests in recognition and vocal reproduction of melodic intervals, immediate memory for intervals and for short melodies, and recognition of the fundamental note of a chord.[1] The method employed in this last test was as follows: a three-clang chord was played, and after it a single low clang, with the question, "Is this the fundamental basic tone of this chord? Does it, in a way, represent the whole chord? If you had to supply a bass to this chord, is this the tone you would use?" Twenty four chords were given, eight in the first position, and eight each in the first and

[1] The writer acknowledges indebtedness to Professor Meyer for the suggestion of this test of musical ability.

second inversions. The low tone which followed was always a lower octave of one of the tones of the chord, and in one half of the instances it was the fundamental. The number of right judgments for each observer is given in the second column of the accompanying Table 3. The percentages in

TABLE NO. 3

Tests of musical ability.

Observers	RECOGNITION OF FUNDAMENTAL OF CHORD				VOCAL REPRODUCTION OF FUNDAMENTAL TONE			
	Right	Wrong	Doubtful	Per cent	Right	Wrong	Doubtful	Per cent
Po........	24	0	0	100	24	0	0	100
Rk........	24	0	0	100	23	1	0	97
Rg........	22	2	0	92	22	2	0	92
Da........	20	2	2	88	23	1	0	97
Ho........	20	2	2	88	17	5	2	75
Fr........	16	4	4	75	18	5	1	77
Ta........	12	8	4	58	12	12	0	50
Mc........	8	5	11	56	10	10	4	50
Pu........	4	0	20	58	—	—	–	—

(In computing percentages, doubtful cases are distributed equally between right and wrong cases.)

the last column represent the success of the subjects in humming the fundamental tone after hearing the chord, the series of chords used being similar to the one employed in the previous test. Errors were most frequently made when the low note was not the fundamental, but was a lower octave of the highest note in the chord. It was found after the series was ended that fewer errors of this kind are made if the observer is instructed not to give his judgment immediately, but first to image the three tones of the chord separately, choose the fundamental, and then make the comparison with the low tone. On repetition of the test, this precaution served to eliminate all errors from the judgments of Rk., Ho., and Da., but did not operate so successfully with those observers whose auditory imagination is less facile.

The results of these tests when combined with the other

observations on musical ability and with the results of an inquiry into the observers' musical interests, their early training and later musical experience, made it evident that the first three observers on the list had a fair order of musical capacity, although Po. was the only one whose abilities had been much developed by training. The last three observers form a distinct group, since they all fall much below the others in the tests reported in Table 3, and also in accuracy of recognition and reproduction of melodic intervals. Pu. could not even be induced to attempt vocal reproduction. The remaining three observers form an intermediate group. None of the nine were entirely lacking in musical interest, although the range represented was a very wide one. An accurate test of ability in pitch discrimination was not carried through to completion because it became evident that accuracy in the discrimination of small differences of pitch is no indication of musical ability. Po. and Rg. did not serve during the preliminary experiments. Da. and Mc. did not serve during the second half year, and their records are included only in the first of the tables presented here. Each observer served for a period of three quarters of an hour once a week.

The observers it will be recalled were directed to take whatever rate of finger movement seemed most natural to them. The individual differences, and also the individual variations from time to time, proved to be extremely wide. Early in the practice experiments, the tapping of Rk., Da., Ta., and Mc. was much slower than it became later on, and nearly all of the observers showed some tendency to increase the natural rate with practice. Within a series of experiments at a single sitting, Rg., Rk., and Mc. were apt to choose a much more rapid rate for the later experiments, unless they happened to select an unusually rapid rate to begin with. This they were apt to do if they had been walking rapidly or otherwise exercising shortly before, or if they had been under any slight excitement.

Not only do the records show great individual differences in the rate of finger movement, but also in the amplitude and

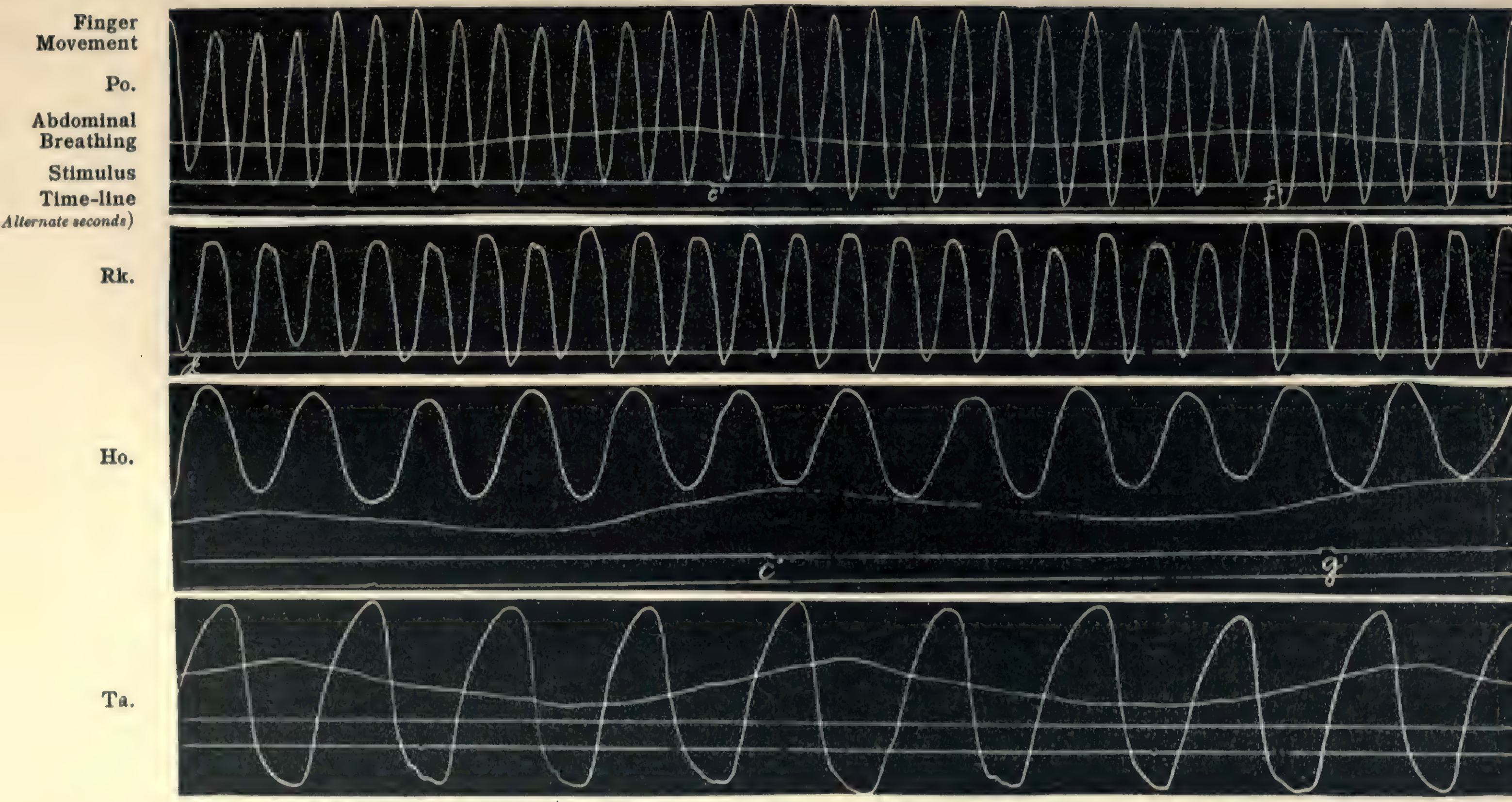


FIG. 2. Reduced one-half.

the general form. Ta.'s record is characteristically slow, wide, and extremely regular. The back stroke is similar to the beat stroke in every respect, and the transitions from the ballistic part of the movement to the controlled portion are smooth and even. The tapping of Da. and Mc. is also slow and wide, but very different from that of Ta. because the ballistic strokes are made with a jerky movement, and the portions of the curve between the ballistic strokes are very irregular. The muscular coördination is much less accurate. Ho. and Pu. also use a characteristically slow rate, but the amplitude of movement is small. One finds very considerable variations in amplitude in the records of both these observers. There is also an irregularity of line due to the fact that the ballistic portion of the movement seems to be almost wholly lacking, even from the beat strokes, (*i. e.*, the finger seems to be almost continuously under control of extensor and flexor muscle sets combined.) The maximum velocity of the beat stroke is much less with these observers than with any of the others. Rk. and Rg. are the two who show the widest variations of natural rate from time to time and also the greatest changes in the form of the finger movement. Both of them use a medium amplitude, but this amplitude varies widely. On the whole, their records show that a much greater prominence is given to the ballistic phase in both beat stroke and back stroke. In Po.'s records, which exhibit the most rapid rates of any of the observers, there is very little in the curve other than the ballistic phase: there is almost no pause between strokes. In the records of Fr., on the other hand, there is always between the vigorous ballistic strokes a relatively long relaxation phase during which the movement is extremely irregular: during these periods the finger seems to be not under the control of either the extensors or the flexors.

With reference to the amplitude of finger movement, it may be noted that with the exception of Ta., those who used a wide amplitude were those who had had some practice at the piano.

TABLE NO. 4

Normal record of rate of finger movement, and fatigue record. Rate of tapping during successive periods of three seconds each. Read from left to right. The slowest rates are printed in bold faced type. Fastest rates in italics.

OBSERVERS	NORMAL RECORD										FATIGUE RECORD		
Po.....	252	256	256	252	254	**249**	252	260	*263*	(One minute of unrecorded tapping elapses between normal record and fatigue record.)	**232**	239	235
Rg.....	*107*	104	106	99	101	99	**92**	100	99		106	104	*108*
Rk ...	130	*133*	133	130	133	132	131	**126**	130		124	124	**121**
Da ...	**97**	*115*	101	105	105	100	103	105	104		99	97	98
Ho ...	*96*	93	90	91	90	91	92	94	**89**		**86**	88	88
Fr.....	208	208	**206**	208	207	210	*220*	217	214		221	*225*	224
Ta	78	78	**76**	77	78	80	79	79	*81*		77	77	78
Mc....	84	85	83	86	87	**82**	*88*	85	86		93	91	*96*
Pu.....	112	**102**	104	107	104	104	109	110	*113*		114	*118*	115

§26 In the accompanying Table 4 are given the measurements of a set of records taken without distraction or stimulus of any kind, for purposes of comparison with records in which melodic stimuli were used. Each number gives the rate of finger movement during a period of three seconds. The rate is expressed in beats per minute, which is the same as the method employed in music for designating rates. The numbers, then, represent the metronome rates at which the observer was tapping during successive periods of three seconds each. To facilitate the reading of the table, the rate of the period of slowest tapping within the record of each observer is printed in bold-faced type, and the fastest rate is printed in italics. A glance at the table will show the extremes between which the rate of tapping varied within the course of the period of twenty-seven seconds covered by the record. It will be seen that four of the nine observers exhibit a tendency toward an increase in rate during this time, while an opposite tendency appears in the records of two observers.

The question naturally arises whether the factor of fatigue may not enter in to modify the nature of the tapping movement as the experiment proceeds. This does not seem to be the case when an experiment does not continue for more

than thirty seconds, as was the case with nearly all of those to be described below. For purposes of comparison, however, there is given in connection with the normal record of the accompanying table what may be called a fatigue record. This is really a continuation of the normal records, one minute of unrecorded tapping having been permitted to elapse between the close of the normal record and the beginning of the fatigue record. During this interval the rate of tapping of four of the observers showed a diminution. With four of the others an increase in rate is seen. The record of Fr. showed the greatest variability and irregularity during this closing period. Only two observers, Da. and Mc., reported any feeling of fatigue after this experiment.

Fatigue makes its appearance very quickly if a rate more rapid than the natural rate of tapping is employed. When the reagent taps as rapidly as possible the entrance of fatigue brings with it a slowing of the rate and an increase in irregularity of rate and of amplitude.

§27 Tables 5 and 6 exhibit the effect of auditory stimuli upon the rate of tapping. These tables are prepared in a manner similar to the table of normal tapping; each number represents the rate of tapping during a three-second period. Measurements of the first few taps of each record were not made because they are certain to be more or less irregular. Measurements of the rate of tapping are given for three periods of three seconds each before the incoming of the stimulus. The stimulus consisted of the tone *a* sounded for six seconds on the harmonium. Then after an interval of three seconds, this tone was sounded again, this time for only three seconds, but it was immediately repeated and sustained for three seconds longer.

A study of this table should disclose the effects which are produced upon the rate of tapping by a musical sound and also by the repetition of a musical sound. It will be noticed that in the records of four of the seven observers there is a marked diminution of rate following the entrance of the first stimulus. The record of one observer shows a marked increase of rate at this point. In all cases there appears to be a tendency

TABLE NO. 5

Effect of a single tone, and of that tone repeated, on rate of finger-movement. The rate during each three-second period of the experiment is given. Read from left to right. Numbers showing decrease in rate at critical points in the record are printed in bold face type; increases in rate are printed in italics.

				a (6 sec)			a (3 sec)	a (3 sec)		
Rk	136	140	141	**123**	*130*	132	132	*135*	132	138
Da	101	109	104	**82**	*96*	80	78	**73**	(97)*	90
Ho	79	81	80	79	78	**75**	**72**	72	71	69
Fr	159	157	157	**155**	155	**153**	**150**	151	150	152
Mc	122	116	118	*128*	125	**118**	121	*127*	129	130
Ta	77	77	76	**69**	*75*	77	76	76	77	80
Pu	60	62	69	66	65	65	66	67	*72*	72

*Stopped tapping for 1.2 sec. when tone stopped; and then began at rate of 97.

TABLE NO. 6

Effect of sudden noise on rate of finger-movement. Entrance of stimulus at beginning of fifth three-second period of the record.

					NOISE			
Po	216	216	214	218	*226*	216	214	210
Rk	150	154	153	152	152	153	160	159
Ho	116	116	114	113	**104**	119	114	116
Fr	202	187	185	190	*198*	194	194	197
Ta	69	72	71	72	**67**	69	70	70

to return to the original rate while the tone is still sounding. The records of three observers show another diminution in rate immediately following the cessation of the stimulus, but no decided change occurs in the other four records at this point. With the entrance of the stimulus the second time a retardation occurs in three records, but this time it is not nearly as large as in the first instance. The repetition of this stimulus is accompanied by an increase in the rate of one observer and a decrease in the rate of another, the rates of the other five observers not changing materially at this point in the records. The cessation of the stimulus, however, is accompanied by an increase in the rate with two observers, and a decrease in a single instance. One observer stopped tapping entirely for a brief time when the stimulus stopped and then began again at a rapid rate.

It thus becomes evident that under the conditions of this experiment the entrance of an auditory stimulus introduces a disturbance in the process of tapping which shows itself as a change in rate, usually of the nature of a retardation. The nature of the disturbance to the tapping is made very evident by direct inspection of the kymographic records. The next tap after the one during which the stimulus enters is frequently the slowest and also has the greatest amplitude of excursion of any tap on the record. The entrance of the stimulus a second time, after a pause, produces similar but much less marked effects; and when no time interval elapses between the clang stimulus and its repetition no effect whatever is apparent.

The effects of a momentary noise as a distraction are illustrated in the experiments summarized in Table 6. Here, too, a marked change of rate appears in nearly every instance. The solitary exception is Rk., and a closer examination of his record than the table permits shows clearly that here too the the stimulus had its effect. The tap immediately following the one in which the stimulus entered is the slowest tap of the record, but in this instance it is followed immediately by taps of a more rapid rate which bring the rate for the entire three seconds up to the figure given.

It seems to be a general tendency, then, for alterations in the natural tapping rate of the finger to occur upon the entrance into consciousness of an auditory sensation. This very natural phenomenon does not call for an elaborate explanation. It may be dismissed by referring it to that large group of experiences which have as their most prominent feature the characteristic of "shock," of sudden disturbance of equilibrium demanding an adjusting act of attention, and which consequently interfere more or less with pre-existing adjustments and on-going activities. Stated in strictly neural terms, the phenomenon is reducible to an instance of the general law of diffusion, the auditory stimulus introducing a shift of neural tensions throughout the cortex, and more particularly affecting those localities in the Rolandic region which are active at the time.

The modification of rate shows itself most frequently as a retardation probably because new activities of adjustment result in inhibition of the finger movement through drainage of the neural energies elsewhere. To explain those relatively infrequent instances (15 per cent of the total number) where acceleration follows the entrance of the auditory stimulus, one might assume that the stimulus operates to produce a greater alertness, or heightened general activity in which the tapping movement shares. To explain why the very first tap following the onset of the stimulus is sometimes unusually wide and of long duration, but occasionally the reverse, recourse may be had to the facts brought out by Hofbauer[1] and Cleghorn[2] that an auditory stimulus occurring at the beginning of the contraction phase of a movement augments the movement and this reinforcement makes the total duration of the contraction-relaxation process greater; but if the stimulus enters at the beginning of the relaxation phase of the cycle, the process of relaxation is hastened and the total period is diminished.

§28 We may now turn to the experiments in which melodic stimuli were employed, asking what significant changes of rate appear, to what extent these variations are the same for the different observers under identical conditions, and especially, what relations exist between changes of rate and the typical phenomena of melody. Do characteristic changes accompany the perception of a melodic interval which is felt to lack finality? How do these changes differ from those produced by an interval which "ends?" Does a succession of two tones which lack melodic "relationship" have a peculiar effect? What of the "return?" What of disappointed expectation? What of the passage to a tone which necessitates a shift of tonality?

Tables 7 and 8 show the changes in rate of tapping which accompany the hearing of the melodic interval of the fourth, *i.e.*, of two tones whose vibration rates are in the ratio 3:4.

[1] L. Hofbauer, *Arch. f. d. ges. Physiol.* (Pflüger's) 1897, *68*, 546.

[2] Allen Cleghorn, "The Reinforcement of Voluntary Muscular Contraction." *Am. Jour. Physiol.* 1898, *1*, 338.

This is one of the most interesting of any of the melodic intervals from a psychological point of view because of the strong sense of finality which it gives when the higher tone is the last. When heard as a descending interval, it lacks this finality, and yet does not leave one wholly in suspense, for it has those elements of finality which are the property of any descending interval as such, and also those which belong to every tone in the tonic chord. Because of this complexity, judgments regarding the finality of the descending fourth are often uncertain and variable. As an ascending interval, however, there is seldom any doubt in the mind of the observer that the group is a completed whole, emphatically coming to an end. It is indeed the only ascending interval of which so broad and positive an assertion can be made. The minor second and minor sixth are the only other intervals at all comparable with it in these respects.

The tables are made up, as were the previous ones, of numbers representing the metronome rate of the tapping movement during successive periods each three seconds in length. The two tones were each sounded for three seconds, and the numbers immediately under the letters which represent the tones consequently express the rate of tapping during the course of the melodic stimulus. To call attention to changes of rate at critical points in the course of the record, use is made of bold faced type where retardations occur, while accelerations are indicated by italics. In deciding whether or not a change of rate accompanying the entrance of a stimulus was sufficient in amount to be of any significance, the writer has taken into account the degree of regularity shown in the tapping of the six seconds preceding, but has neglected the period before that, which was often so near the beginning of the tapping record that the reagent had not as yet found his pace.

Examining Table 7 with reference to the distribution of retardations and accelerations during and immediately following the melodic stimulus, one notices at once that the retardations all occur during the sounding of the tones (six during the first tone and two during the second) whereas all the accelerations are found within the period of the last tone and the

period immediately after it (two during, and six after, the last tone).

In contrast with this table of the ascending fourth, the table of the descending fourth exhibits much less uniformity in the distribution of accelerations and retardations. The most striking feature is the large proportion of retardations which occur during or immediately after the sounding of the second tone.

TABLE NO. 7

Perfect Fourth, ascending. Rate of tapping during successive periods of three seconds each. Read from left to right. Bold faced type indicates retardation and italics acceleration, at critical points.

				c'	*f'*			
Po	207	208	212	212	*222*	225	223	220
Rg	94	95	94	**91**	92	*96*	96	93
Rk	101	106	104	**102**	**99**	*101*	96	99
Ho	105	103	103	**92**	93	*101*	102	100
Fr	190	192	186	**180**	**172**	*185*	179	182
Ta	76	75	86	**73**	72	*87*	78	78
Pu	118	117	120	**112**	*118*	*122**		

*Stopped tapping

TABLE NO. 8

Perfect Fourth, Descending.

				f'	*c'*			
Po	248	255	258	**253**	258	**250**	256	258
Rg	91	93	95	96	97	96	95	94
Rk	104	101	98	*103*	103	**101**	101	103
Ho	95	97	99	99	*103*	104	100	101
Fr	220	214	219	**213**	**210**	*218*	213	218
Ta	82	84	85	**80**	**74**	73	77	80
Pu	100	106	105	105	**101**	*116*	114	116

§29 The significance of these facts appears when they are brought into comparison with the results of the previous group of experiments. There it was found that a repetition of a musical sound following shortly after the cessation of the original stimulus produces effects similar to those of the first sound, but much less marked. And when one musical sound is immediately followed by another which does not differ from it in

pitch or intensity there is no apparent effect upon the on-going activity, the only changes observable being in the direction of a return to the natural rate.

When successive tonal stimuli differing in pitch are used—in this instance two tones at an interval of a fourth—the characteristic variations of rate, most of them retardations, follow the entrance of the first tone; but when this is succeeded by the second tone, one does not find the same absence of further variations which marked the appearance of a second tone identical in pitch with the first. Instead one finds fresh changes of rate; and upon comparing the ascending fourth with the descending fourth one is impressed with the fact that the accelerations belong mainly to the rising interval, while most of the new retardations accompany the hearing of the descending fourth. This, it will be born in mind, is an interval that "ends" better on the higher tone.

An hypothesis with reference to the significance of these motor phenomena may here be briefly outlined, as follows: (*a*) Attention is an activity which involves both special and general motor adjustments. (*b*) The general aspects of attentive activity are of such a nature as to affect general bodily conditions; and, specifically, (*c*) the rate of a circular motor process (such as the finger-movement) which is going forward semi-automatically, will be affected by these activities, a decrease in rate signifying inhibition, due to increased activity elsewhere, and an acceleration signifying that the task of attention in organizing these activities is being successfully carried out. Retardation or inhibition, it is to be expected, will enter with the appearance of the stimulus demanding attention. Continued slow rate of movement will result if the organizing activities of the attentive process continue to meet with difficulties, while the rate will be augmented as the new adjustments come to be efficiently established.

In terms of this hypothesis, the above facts with reference to the hearing of the rising fourth would be described as follows: Sudden rise in the level of attention at entrance of stimulus, continued attentive activity during the sounding of the tones, and finally, subsidence of attentive activity with the

satisfactory completion of its task; or, stated differently, presentation of a problem of adjustment as stimulus enters, continuance of the process of establishing coördination during the sounding of tones, and then increase of rate signifying the efficient accomplishment of this act.

It is this acceleration accompanying the sense of finality which seems to be of particular significance.

§30 In testing the hypothesis, the introspections of the observers must be taken into consideration, for not always is a melodic interval heard in the same way. What an interval is to the observer depends as much upon the "attitude" with which it is received as it does upon the ratios of the physical vibration rates.[1]

The order of arrangement of the observers in all the tables, it will be recalled, is that determined by the tests of musical ability. Po., the most musical, reported that the ascending fourth, while it has the attribute of finality, is less final than some, *e.g.*, the descending fifth.

> "The pitch of the second tone came as a surprise. The feeling of satisfaction came only toward the end of the second sound, after I had got it placed with reference to the first. The instant of entrance of the sense of satisfaction was very marked."

(The rate for the first four taps of this period was 210, for the next four it was 228 and for the remaining three, 232.) This experience might be described as the final acceptance of a second tone as a tonic which when first heard was not so construed. If, during the hearing of the first tone, a tonality feeling gets established with this tone as a tonic—as is very frequently the case—the transition to a tone of different pitch presents three possibilities. (*a*) It may be an "unrelated" tone, foreign not only to the tonality already in mind but also to any other tonality within which the first tone would find a place. In such an instance there can be no melody feeling,[2] for there is no coherence or relevance between the tones; they do not tend to insti-

[1] *Cf. supra*, p. 32 *ff*.

[2] Here, and throughout the discussion of the experiments, it will be understood that these statements are made solely with reference to the experience of observers who are familiar with a harmonic musical system.

tute a common set of expectations; they do not belong to the same whole. (*b*) The second of the two tones may be "related" to the first as to a tonic. It belongs to the tonality already in mind, and consequently it is welcomed, as partially satisfying the expectations of the hearer; but it does not wholly satisfy them. Instead, it only makes more definite and insistent the demand that the first tone shall be heard again, at the end of the melody; it intensifies the original tonality feeling. If the sequence of tones ends here, one experiences the feeling of unrest and dissatisfaction which accompanies disappointed expectation or thwarted intent. (*c*) The second tone may be capable of entering into tonality relations with the first, but not into the tonality of which that tone is the tonic. This necessitates a shift of tonality. In place of the organized set of expectations already present, a different set appears. The extreme instance of this peculiarly subtile and elusive process occurs when the second tone becomes itself the tonic of a new tonality, usurping the power and function originally held by its predecessor, and organizing a new set of expectations. Such an instance is found in the interval of the ascending fourth.

Po. was probably not the only observer who experienced this peculiar shift of tonality upon hearing the interval of the ascending fourth; but he is the only one who detected and described the feeling of transition and the satisfaction which followed. Rg. reported that the interval seemed to him to be rather indifferent, but after hearing *f′c′* he said that *c′f′* had more finality about it than he had thought at first. Rk. reports, "That sounds like '*sol do*'; there is no need of a third tone." Ho. "That ends! It is very agreeable." Fr. "That's all right." Ta. found it difficult to give an introspective report. The interval he said was elusive, and it was hard to say just what the effect was. Pu. reported no definite effect of any sort. It must be noted that even in the case of these last two observers an acceleration of rate occurred immediately after the close of the tone.

With the descending fourth we find much less uniformity in the distribution of accelerations and retardations, and also a greater diversity in the introspective reports. The most

striking and important feature is the large proportion of retardations which occur during or immediately after the sounding of the second tone. Po. reports that the interval was pleasing, but not wholly satisfactory because it lacked finality. During the sounding of the second tone his rate recovered from the slowing-up produced by the first tone but after the melody ended there was a retardation. For Rg. the interval lacked finality but as to agreeableness it was indifferent. Rk.'s introspections were interesting. "That is all right, but I can't help thinking in three's." That is to say, he gave an intellectual judgment that the interval was complete but really felt a need for something further. (Note the retardation in rate.) Ho. says, "I should like to add a third note but it is not bad." Fr., "Unfinished, but pleasant as far as it goes." Ta. "I cannot decide. I keep changing my mind. It is a puzzling interval." Pu., "Very definitely complete and pleasant."

If one examines the table in the light of these introspective comments, it is found that five of the seven records support our hypothesis with reference to the motor effect of the finality experience.

With all of the remaining tables the introspections are presented in very brief summary. The observer's own words are used, as far as the necessities of condensation allow.

§31 Tables 9 and 10 should be examined together. They show the effects produced by the melodic interval of the perfect fifth, ascending and descending. With regard to the aspect of finality, all the observers with the exception of the two least musical ones are agreed that the ascending fifth is lacking in completeness. In spite of this fact, the proportion of retardations and accelerations during the period while the second tone was sounding and immediately after, do not show a balance in favor of the retardations. The lack of finality in this interval is not sufficiently marked to produce the vivid experiences of tension which characterize the perception of some melodic intervals. A more significant reason why one should not expect a larger proportion of retardations here, will become evident shortly.

TABLE NO. 9

Perfect Fifth, Ascending. Rate of tapping during successive periods of three seconds each. Read from left to right. Numbers showing decrease in rate at critical points in the record are printed in bold face type. Increases in rate are printed in italics.

				c'	g'			
Po	225	224	225	224	*228*	*236*	236	230
Rg	129	130	127	126	125	*132*	124	128
Rk	117	117	118	119	**116**	115	114	118
Ho	102	110	111	**104**	**96**	*102*	105	104
Fr	234	233	235	*237*	**231**	232	226	230
Ta	73	74	76	**71**	*77*	*80*	81	83
Pu	102	109	108	**105**	*111*	**104**	103	

Introspections.

Po. A sense of finality, but not completely final. Pleasant.
Rg. A beginning, not an end. Wanted to go on.
Rk. Want to hear first again.
Ho. Needs third tone. Not extremely bad.
Fr. Unfinished. Pleasant.
Ta. That is finished! Felt so the instant it sounded.
Pu. Fairly complete. Agreeable ending, but I do not like so wide an interval.

TABLE NO. 10

Perfect Fifth, descending.

				g'	c'			
Po	197	204	208	**204**	*208*	*214*	219	220
Rg	129	125	135	132	**125**	*134*	133	143
Rk	106	108	108	**102**	101	*105*	103	105
Ho	109	113	111	**107**	106	*109*	*	
Fr	234	225	220	221	220	220	222	229
Ta	78	78	78	78	78	*83*	83	82
Pu	103	106	113	**101**	**97**	*112*	104	101

*Stopped Tapping

Introspections.

Po. No suggestion of further movement. Satisfactory.
Rg. Left no impression.
Rk. Doesn't need a third. Pleasant.
Ho. Can't say as to finality. Fairly agreeable.
Fr. Incoherent. Unfinished. Unpleasant.
Ta. (Introspection uncertain.)
Pu. Did not demand third note.

Table 10, the descending fifth, presents a much more uniform appearance. Accelerations following the close of the melody occur in every record except that of Fr., which shows no change in rate at this point. The introspections, however, are not as definite, three observers failing to report anything positive regarding the finished, self-complete character of the melody. The only one, however, who found the melody incomplete was Fr., the observer whose rate is the only one to show no increase at this point.

TABLE NO. 11

Perfect Fifth, descending. Three tones expected. Average rate of tapping by three-second periods. Read from left to right.

				g′	c′			
Po.	284	284	275	277	275	**267**	269	269
Rk.	205	202	206	**202**	204	**194**	197	223
Rg.	112	117	117	**113**	112	*128*	118	127
Ho.	108	110	111	111	**105**	**101**	99	100
Ta.	76	76	77	**68**	*70*	*73*	75	73
Pu.	104	108	108	**106**	*108*	**100**	101	104

Introspections.

Po. Amusing. Incomplete.
Rg. A feeling of incompleteness.
Rk. Disappointing.
Ho. Unfinished, because of expectancy of another tone.
Ta. Incomplete. Thought you were trying to fool me.
Pu. Surprised that there were not three. Incomplete.

The records from which Table 11 were prepared were taken at the end of the year's experimenting because it was desired to avoid the suspicious attitude which it might possibly have induced in some observers. One of the details of method, it will be recalled, was to let the observer know beforehand how many tones were to be expected, in order to keep the conditions in this respect as constant as possible. In this final experiment, however, the observer was led to expect three tones, but only two were given, the same two used in the experiment just described. (Table 10). Any changes in rate of tapping produced by *unfulfilled expectation* ought then to become evident by a comparison of these two tables, 9 and 10, and indeed

the difference is sufficiently striking. Instead of uniform accelerations following the tones one finds retardations in nearly every instance.

This, then, may aid us in understanding the accelerations so frequently found where introspection reports that the interval lacks finality. As a melodic interval it is left unfinished, but in so far as the hearer was expecting a certain number of tones and that expectation was fulfilled, the experience as a whole gets a certain completeness and unity. Part, at least, of the adjustments of attention have functioned as intended, and only so much of the total motor attitude as was immediately concerned with the tonality experience as such has to be re-adjusted when the melody comes to an end on what is not a final tone.

The diminished fifth (45:64) was selected as an example of a group of two "unrelated" tones. The testimony of the observers is nearly unanimous that the interval lacks completeness and is disagreeable to hear both ascending and descending. (Tables 12 and 13.) Nevertheless there are a larger number of accelerations than of retardations. A comparison of the "exceptions" with those in the introspective table clears up the difficulty somewhat, but even then it must be said that this pair of tables tells against our hypothesis. The only recourse

TABLE NO. 12

Diminished Fifth, ascending. Average rate by three-second periods. Read across.

				b	*f'*			
Po.	265	274	277	**270**	267	270	270	265
Rg.	122	114	118	*122*	*126*	**117**	116	115
Fr.	247	232	232	*242*	**233**	*247*	239	240
Ta.	76	80	78	**72**	73	*76*	78	77
Pu.	74	73	75	**65**	*76*	*79*	79	84

Introspections.

Po. A raw rough interval. Associations with Wagner made it less disagreeable. Incomplete.

Rg. Disagreeable because incomplete.

Fr. Not finished but good as far as it went.

Ta. Unfinished but a pleasant interval.

Pu. Very disagreeable. Felt at entrance of second tone.

TABLE NO. 13

Diminished Fifth, descending. Average rate by three-second periods. Read across.

				f′	*b*			
Po.	263	276	265	263	267	*274*	270	267
Rg.	116	118	117	115	*131*	130	131	119
Fr.	192	200	219	207	**198**	**192**	202	211
Ta.	76	78	76	**70**	71	*76*	74	76
Pu.	80	79	77	79	**76**	*82*	85	77

Introspections

Po. Incomplete, but not seriously so.

Rg. One more tone (he hummed *c*) would make a great difference.

Fr. Very unpleasant. It seemed complete because you told me there would be but two.

Ta. Finished. A pleasant interval.

Pu. Didn't think about completeness. At first thought it disagreeable, then not sure.

is to the principle that the tapping tends to become rapid whenever attention is freed from the stimulus, irrespective of what the stimulus may be.

The descending major third is an emphatically final melody (although Fr. and Pu. did not so describe it), and the table (No. 14) shows the expected accelerations. The most interesting feature is, however, the marked retardation in the record of Rk. The last tone was a final tone, he said, but he wanted a third tone in *between* the first and second, and tried to figure out what tone that should be. The retardation occurs in the portion of the record where this was being done.

In this and several of the following tables are given the measurements of a single record in which the rate of each separate tap is determined. Samples of the tapping of each of the different observers are thus made available for detailed inspection. It is interesting that the rate for individual taps can fluctuate as widely as it does without greater variability in the rate as measured for periods of three seconds.

The minor sixth (5 : 8) was, somewhat to the surprise of the experimenter, judged to be an incomplete and disappointing melody, ascending as well as descending. It has the

TABLE NO. 14

Major Third, descending. Metronome rate of each separate tap. Read down.

				e′	c′			
Rk.	177	187	202	198	218	178	148	172
	181	163	206	191	202	149	148	182
	168	182	202	185	153	148	246	185
	177	160	179	191	171	159	198	188
	181	164	185	211	176	162	182	171
	176	182	160	209	182	148	153	169
	182	153	190	271	163	159	191	158
	177	171	197	202	132	148	185	172
	182	182	166	226	183		183	148

Average rate for each three-second period. Read across.

Rk.	178	171	185	*207*	**188**	**162**	176	171
Po.	225	222	225	227	229	*237*	230	237
Ho.	100	103	104	**98**	97	*106*	106	102
Fr.	193	195	209	205	206	*213*	195	214
Pu.	80	81	85	**77**	80	*88*	84	86

Introspections.

Rk. Wanted a third tone *between*. Tried to decide what it should be.
Po. Surprising, but very satisfying. Final.
Ho. It became satisfactorily complete after I had thought about it.
Fr. Coherent, but suggested something further.
Pu. Needed a third tone to complete it;

TABLE NO. 15

Minor Sixth, descending. Metronome rate of each separate tap. Read down.

				g′	b			
Ho.	106	103	113	119	109	105	92	111
	117	106	115	104	89	100	103	119
	82	96	105	101	110	102	114	106
	110	117	123	106	128	101	97	102
	111	110	111	110	111	89	106	89

Average rate by three-second periods. Read across.

				g′	b			
Po.	225	220	224	**204**	*222*	222	225	227
Rk.	137	128	135	139	*145*	**130**	130	137
Ho.	104	106	113	108	108	**99**	101	105
Fr.	172	158	181	175	183	183	184	182
Ta.	106	101	105	105	107	**100**	103	105
Pu.	112	111	112	114	*127*	**118**	122	125

Introspections

Po. Surprise and disappointment on second tone. Unsatisfactory.
Rk. Does not end.
Ho. Very noticeably lacked finality.
Fr. Quite unrelated.
Ta. Tone pleasant but melody does not end.
Pu. Unsatisfactory. Incomplete.

TABLE NO. 16

Minor Sixth, ascending. Metronome rate of each separate tap. Read down

				b	*g'*			
Ta.......	80	86	75	81	77	79	79	81
	86	81	83	72	82	77	79	79
	82	81	83	72	79	81	81	82
	81	82	82	73	71	86	81	75

Average rate by three-second periods. Read across.

				b	*g'*			
Rg.......	99	101	104	**98**	101	**97**	99	100
Ho.......	102	108	110	**102**	**98**	98	98	100
Fr.......	208	207	215	*223*	**215**	**208**	201	213
Ta.......	82	82	81	**74**	77	*81*	80	79
Pu.......	97	97	97	**95**	95	*100*	102	107

Introspections

Rg. No melody; no finality.
Ho. Seemed bad at first but changes to a final interval.
Fr. Unconnected and therefore unpleasant.
Ta. Incomplete.
Pu. Unrelated. The second note seemed to change in character.

character of incompleteness very strongly as a descending interval, but when heard in the opposite direction it is possible so to reconstruct the tonality as to make the higher tone a tonic. This, the observers, with a single exception, failed to do.

Consequently Tables 15 and 16 may both be taken as showing the effects of a melody that lacks finality. The unusually large number of retardations strikes the eye at a glance.

§32 Turning now to some examples of three-tone groups (tables 17 and 18), we are confronted at the outset with the

difficulty that it is usually quite possible to interpret any group of three related tones in a variety of ways, and we are thrown back upon the introspections of the observers for a starting point in our interpretation of the results. This method has its obvious disadvantages, notably those resulting from the probably imperfect reports which the average observer can give about so complex an experience as the course of a three-tone melody.

TABLE NO. 17

Three-tone groups. Average rate for each three-second period. Read across.

				g	e′	c′			
Rk........	140	143	141	142	141	*146*	*153*	147	142
Ho........	122	118	116	**109**	*117*	118	116	118	112
Ta........	71	74	71	70	*76*	*87*	88	75	72
Pu........	123	120	135	**114**	*128*	127	127	121	138

Introspections

Rk. Finished. Very good melody.
Ho. Complete, satisfactory.
Ta. Incomplete.
Pu. Uncertain.

TABLE NO. 18

				g	e♭′	b♭			
Rk........	130	131	126	*132*	*139*	*151*	*170*	139	136
Ho........	110	118	113	112	*115*	**111**	*118*	112	118
Ta........	70	70	68	**63**	*67*	*76*	**66**	66	66
Pu........	145	144	154	**138**	142	*148*	**141**	146	151

Introspections

Rk. Leaves me in suspense.
Ho. Unfinished. Don't like it.
Ta. Second note did not fit in at all. Very disconnected.
Pu. Fairly good ending, but the intervals are too wide.

The two melodies placed together here for comparison are very similar in form, and both are made up of wide, consonant intervals, but one of them, the first, seemed to the experimenter to have a more positive finality. The more musical

observers agree with him in this. All of the retardations (neglecting of course those which accompany the entrance of the first tone) occur at the end of the less final of the two melodies.

On the whole these tables are not very illuminating.

TABLE NO. 19

Three-tone groups. Average rate for each three-second period. Read across.

				c′	*a*	*b*			
Rk........	154	152	160	**154**	*159*	174	**166**	179	160
Ho........	115	107	109	*112*	**105**	**86**	*96*	110	104
Ta........	69	71	71	70	71	72	70	70	71
Pu........	103	105	105	**96**	*102*	101	*109*	114	114

Rk. Unsatisfactory. *Must* go back to first tone.
Ho. Perfectly horrid! Due to the last tone.
Ta. Could give no introspection. (Note regularity of rate.)
Pu. Indifferent.

TABLE NO. 20

				c′	*a*	*b♭*			
1 Rk......	164	160	159	*173*	**168**	*180*	**170**	177	168
2 Rk......	159	156	157	**149**	145	146	*155*	171	168
3 Rk......	178	180	184	**179**	180	*187*	188	181	181
Ho........	97	102	106	104	**99**	*103*	101	100	104
Ta........	77	80	77	**73**	75	*87*	85	78	85
Pu........	97	99	98	*100*	**97**	*111*	**106**	99	98

1. Rk. Wrong, but not very bad. Second note spoiled it.
2. Rk and 3 Rk. (repetitions at a later date of same tones.) Both satisfactory and complete, the latter reassuringly so.

Ho. Last note predominates and becomes satisfactory ending.
Ta. Indifferent ending. Last note a disappointment.
Pu. Tones seemed disconnected.

Table 20 is of interest mainly because it shows the different reactions which the same melody elicited from one of the subjects at different times. The group of intervals, c′-a-b♭, is one which demands a shift of tonality, but which then ends, satisfactorily. When it was first given, Rk. did not so hear the melody: the tonality did not become readjusted. Two weeks later the experiment was repeated and this time the tones were heard as a complete melody. It was immediately given again, with similar but more positive introspective

reports as the result. The three records show the expected differences in the tapping.

A striking record is that of Ta. (Table 19). He tapped throughout the course of the experiment almost with the regularity of a ruling engine. When asked for an introspective report, he could find nothing to say! The tones had had no effect whatever.

Every retardation shown in these tables finds its explanation in the introspective records. Not quite as much can be said for all of the accelerations.

With table 21 we take up the study of the "Return." The interval here used is the major second (8: 9). This is a very satisfactory melodic figure when the lower tone is the start-

TABLE NO. 21

Three-tone group. Major second. Average rate for each three-second period. Read across.

				d′	*c′*	*d′*			
Po........	248	244	249	**242**	**218**	*229*	*244*	253	251
Rk........	192	195	191	**171**	170	170	*176*	183	187
Ho........	109	101	102	**93**	*96*	**92**	*98*	98	102
Ta........	100	107	112	**103**	**98**	*102*	**99**	101	101
Pu........	95	94	98	**89**	91	101	*105*	101	103

Po. Second tone very unpleasant. Third reinstated calm and repose of the first. At loose ends on second. The return changed all this.

Rk. Very unsatisfactory as a whole but had a certain unity about it.

Ho. I think that ended nicely. It is curious that I can not recall the middle tone.

Ta. The lower would have been a better ending.

Pu. Second note not right. Return to first gave feeling of finality.

TABLE NO. 22

				c′	*d′*	*c′*			
Po........	259	265	265	**259**	**254**	*261*	**256**	265	252
Rk........	162	188	181	**164**	**158**	*171*	*178*	192	181
Ho........	114	105	110	**97**	*100*	**93**	*100*	93	
Ta........	114	106	101	*107*	**95**	*102*	*102*	121	115
Pu........	106	118	102	*111*	**96**	95	*108*	100	102

Po. Third tone a pleasant relief from suspense.

Rk. It was all right at the time.

Ho. Very pleasant and complete.

Ta. Positively finished.

Pu. Pleasant and complete.

ing point and the end, and one is not surprised to find a large proportion of accelerations at the close. (See table 22, c'-d'-c'.) The record fits well with the introspections.

When the upper tone is made the point of departure and return, the melody tends to fall apart. The middle tone positively will not fit into any tonality suggested by the first. This appears very prominently in the introspective records. Another feature is that *without exception* the observers felt that the return from this lower tone to the upper was very satisfactory. "The third reinstated the calm and repose of the first," etc. The entire set of introspections accompanying this table is recommended for careful perusal as clearly setting forth the result of a return from a tone felt to be foreign to the first. The experience acquires a unity which is most certainly *not* contributed by any interval "relationship."

TABLE NO. 23

Three-tone groups. "The Return." Average rate for each three-second period. Read across.

				c'	f'	c'			
Po........	285	260	254	*259*	**244**	**238**	*248*	256	258
Rk........	154	162	164	*168*	**160**	*171*	*178*	176	167
Ta........	82	83	81	**76**	*79*	78	**74**	76	84
Pu........	99	105	110	**96**	*110*	**99**	*113*	107	108

Introspections.

Po. Much less complete than if upper tone were last.
Rk. Satisfactory ending, but not so good as f-c-f (hummed).
Ta. Finished.
Pu. Incomplete. Second tone unrelated to others.

TABLE NO. 24

				f'	c'	f'			
Po........	249	249	249	**236**	234	*254*	*259*	261	262
Rg........	119	126	126	**123**	*127*	**123**	*126*	128	126
Rk........	171	180	183	**172**	170	*176*	176	181	
Ta........	79	76	75	**72**	70	*73*	72	75	76
Pu........	81	86	87	*90*	**86**	*90*	*101*	97	96

Po. Emphatically final.
Rg. O. K. Finished.
Rk. Fairly satisfactory. More so than c-f-c (hummed).
Ta. Fairly complete.
Pu. Complete.

TABLE NO. 25

Three-tone groups. "The Return."
Rate for each separate tap. Read down.

				c'	g'	c'			
Po........	243	274	242	240	267	236	248	236	252
	285	267	246	253	236	254	246	229	267
	262	258	226	260	240	244	276	204	267
	265	260	226	247	256	252	265	223	246
	258	222	239	252	276	254	263	213	242
	260	232	221	262	262	269	236	229	254
	272	252	224	248	267	278	247	256	254
	265	254	253	233	260	250	260	265	243
	260	236	236	233	231	250	252	260	267
	276	221	236	252	269	248	233	256	240
	272	224	258	276	277	224	270	250	272
	269	232	242	272	256	228	260	258	253
	272	226	240	272	258	240	234	246	254

Three-tone groups. "The Return." Average rate by three-second periods. Read across.

				c'	g'	c'			
Po........	268	238	238	254	258	248	251	240	254
Rk........	118	118	119	**116**	118	*128*	**115**	117	
Ho........	79	80	79	78	80	*82*	*93*	95	95
Fr.........	208	203	200	199	196	*206*	207	200	198

Introspections.

Po. More or less complete. Not very good.
Rk. O. K. Finished.
Ho. Complete. Very pleasant.
Fr. Complete, but not wholly satisfactory.

A study of the table of rates itself is equally illuminating. In number and distribution of accelerations, it is almost identical with the companion table, where the return was from a tone felt to be quite coherent with the first tone of the melody.

Tables 23–26 also show the effects of the return to the starting point. The intervals used differ from the preceding in that they are wider, and consonant intervals. The fourth (tables 23 and 24) ends more emphatically upon the upper note, the fifth (tables 25 and 26) on the lower. This was the judgment of the observers. The small sprinkling of retarda-

tions at the close of these melodies would indicate that this difference in finality is unable to maintain itself,as against the two factors that tend to exert an opposing influence upon the tapping, the factors, namely, of the return, and of the fact that the expected number of tones was heard and nothing further anticipated.

TABLE NO. 26

				g'	c'	g'			
Po.........	230	230	237	**227**	**208**	*239*	*246*	251	
Rk........	138	138	140	**131**	*140*	139	**136**	128	
Ho........	77	78	80	79	**77**	*80*	***86***	84	
Fr.........	204	197	196	197	**193**	195	193	196	203
Ta........	74	74	80	74	73	*76*	*81*	75	81
Pu........	106	105	107	**103**	102	*116*	*119*	110	108

Po. No feeling of finality; therefore unpleasant. No tendency to go elsewhere.
Rk. Not as complete as c'-g'-c' (hummed), but one isn't left in suspense.
Ho. Can't say as to completeness. Unpleasant.
Fr. Incomplete.
Ta. Better to end on second note.
Pu. Not emphatic finality; only such as any 'return' gives.

What of the octave? Meyer was unable to detect any stronger "trend" to the lower than to the upper tone, and consequently put himself on record as opposed to Lipps and the other writers who assert that the lower tone possesses the stronger finality.[1]

The question was put to each of my observers. They were asked to judge with reference to the finality of ascending octaves, descending octaves, and also groups of three tones, involving the return. Intervals in the middle region of the scale and also in the great octave were used. The results were strongly against Meyer's view. Pu., the least musical of the observers, could detect no difference in finality between the end on the upper and the end on the lower of two tones an octave apart. All others found that a stronger feeling of finality attached to the end on the lower tone. This dif-

[1] *Psych. Rev.* 1900, 7, 248. In the light of his more recent studies on the effect of the falling inflection (see above, p. 28) we suspect that Meyer would today formulate somewhat more guardedly his statements regarding the psychological effect of the close on "1" and on "2."

ference of preference does not make itself evident, however, in the tapping records of tables 27 and 28 (the octave). At the close of the melody there is found almost exactly the same preponderance of accelerations over retardations in each of the two tables. Although one ending is better, both are good.

TABLE NO. 27

The Octave. Rate of each separate tap. Read down.

				c'	c''	c'			
Rg........	90	93	96	88	83	70	81	85	86
	81	83	93	85	93	83	92	93	90
	88	88	95	93	81	88	97	83	83
	93	88	88	81	79	81	86	90	85

Average rate for each three-second period. Read across.

				c'	c''	c'			
Rg........	88	88	93	**86**	**83**	85	*88*	88	86
Po........	258	252	254	**245**	**238**	239	*257*	256	284
Rk........	238	232	236	**211**	**204**	205	*210*	218	231
Ho........	120	120	119	**104**	*108*	109	**96**	107	108
Fr........	186	199	199	*205*	*216*	**207**	206	218	210
Ta........	78	81	78	**72**	71	73	*77*	81	79
Pu........	96	93	98	*109*	112	**101**	*112*	120	113

TABLE NO. 28

The Octave. Rate of each separate tap. Read down.

				c''	c'	c''			
Rg........	93	96	91	76	95	90	94	88	98
	95	91	93	93	85	99	99	102	92
	90	90	93	89	88	88	90	92	93
	96	99	94	88	89	85	86	91	90

Average rate for each three-second period. Read across.

					c'	c''			
Rg........	93	94	93	**86**	*90*	90	*92*	93	93
Po........	272	261	277	**245**	**232**	*262*	**254**	256	252
Rk........	251	250	259	256	*262*	260	259	268	266
Ho........		107	108	**102**	**97**	*104*	*106*	111	106
Fr........	198	199	192	*215*	216	**207**	205	206	212
Ta........	81	81	81	**78**	*87*	82	80	81	82
Pu........	130	138	116	112	114	114	*124*	120	147

§33 In the last two tables to be presented, Nos. 29 and 30, are shown the rates of tapping during the hearing of a longer group of tones. Here the exact number of tones was not told in advance, the observers being informed merely that they might expect several more than the usual number. The two "melodies" are alike in that they both start and end with "c," and both use the same intermediate tones; but they differ in the order of these tones. The first group moves slowly but naturally forward, and at length comes inevitably to rest on the last of the seven tones. The second moves as slowly and as regularly, and reaches the same goal,—and yet the goal is not the same. Subjectively it is no goal at all. None of the observers knew when it had been reached until the tones abruptly ceased, whereas with the previous group, all but one reported that they knew the last tone was the last as soon as it began to sound. The first sequence, then, is a genuine melody; the second is not.

One or two typical introspections may be quoted as representative of the sort of experience which was more or less common to all of the observers. Rk. (first seven-tone group.)

> During the first three notes I did not know what was the melodic meaning or general direction, but on the fourth note it took shape and I anticipated what the next would be, and so on to the last. The last was definitely final. It didn't occur to me that there might have been more tones until you suggested the possibility of it.
>
> (Second group.) The third note was not what I expected. The sixth would possibly have made a good ending. The last note was a disappointment; it wasn't offensive, but obviously was not the best possible.

None were satisfied with the ending of the second group of tones; all thought it more or less incoherent throughout and hard to grasp. But with the first group every observer with one exception was sure, when the last tone had been reached, that that was to be the final tone. The one exception, Pu., could not give a definite answer to the question whether the ending were a surprise or not, whether or not anything further was anticipated.

TABLE NO. 29

Group of tones judged to be a melody. Rate of each separate tap. Read down.

				c'	e'	g'	e'	f'	d'	c'			
Po.....	252	238	236	238	252	228	221	238	258	228	250	248	236
	246	228	235	240	228	220	222	222	256	204	256	236	238
	250	237	236	237	208	220	236	240	240	222	260	256	254
	230	236	237	246	208	233	218	238	251	186	233	246	238
	250	238	238	220	205	211	220	222	246	219	218	256	220
	228	236	232	221	229	220	252	261	236	254	241	254	236
	236	250	256	220	206	233	233	228	246	212	218	236	236
	237	246	258	211	254	220	231	257	228	234	244	236	238
	246	237	252	205	237	224	237	237	245	238	256	254	238
	227	254	257	217	220	217	220	254	256	226	242	234	236
	245	250	252	232	212	203	220	238	238	246	240	234	236
	236	238	257	226	210		220	222	220	236	246	236	244

Average rate for each three-second period. Read across.

				c'	e'	g'	e'	f'	d'	c'			
Po.....	241	244	247	**227**	**221**	219	*227*	*241*	242	**227**	*251*	244	239
Rg.....	98	98	92	95	**90**	89	*97*	**91**	**88**	*93*	*95*	96	94
Rk.....	200	190	196	195	**181**	180	*190*	191	*198*	*207*	205	208	215
Ho.....	87	80	87	80	78	83	82	82	79	*88*	*90*	92	88
Fr.....	205	229	228	222	223	216	**202**	*206*	*212*	212	*216*	206	216
Ta.....	69	70	68	67	**63**	*74*	**68**	66	58	*67*	68	66	69
Pu.....	102	98	105	**97**	*101*	*110*	*114*	**107**	108	**102**	*106*	109	112

TABLE NO. 30

Seven-tone group judged not to be a melody. Average rate by three-second periods. Read across.

				c'	f'	d'	g'	e'	f'	c'			
Po.....	260	254	261	**255**	*276*	**253**	*280*	**238**	*264*	**247**	*260*	253	265
Rg.....		104	108	**103**	*109*	*112*	112	*140*	**114**	**102**	101	105	108
Rk.....	152	153	162	*169*	*172*	168	168	**163**	*169*	**157**	**152**	153	171
Ho.....	102	112	111	**100**	100	102	**99**	101	*106*	*112*	**105**	110	102
Pu.....	117	105	122	117	**110**	*122*	121	**113**	115	*118*	118	125	122

In the tables the changes of rate are shown throughout the course of the melody, but the ones which are of special significance for our purposes are of course those accompanying the strongly contrasted feelings at the end of the tonal sequences. At the close of the first, every record reveals an acceleration in the rate of tapping. In marked contrast are

the retardations found at the close of the other sequence. (See accompanying graph, Fig. 3.)

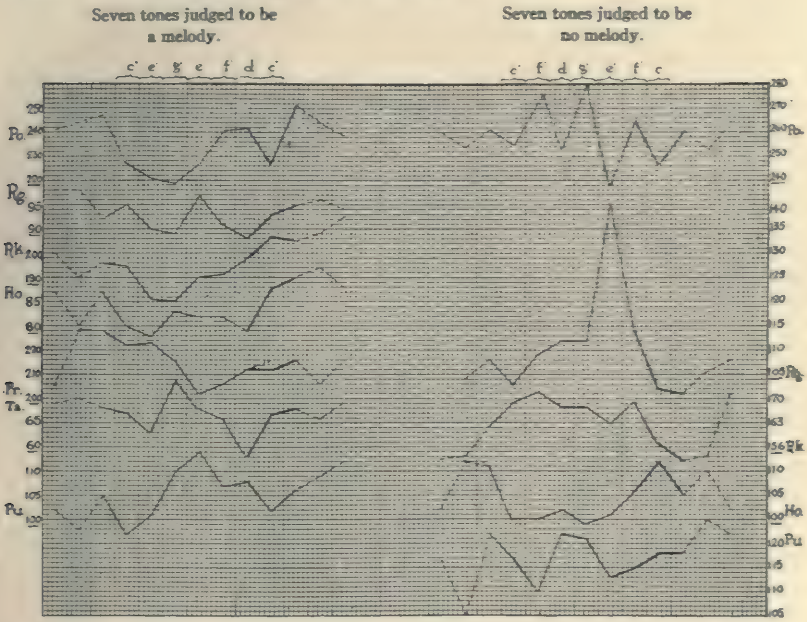


FIGURE NO. 3. EFFECTS OF A MELODY AND A NON-MELODY CONTRASTED. Each tone sounded for three seconds. Graphs represent rate of tapping during each of these three-second periods. Note general tendency toward increase in rate at close of melody, and absence of such acceleration at close of non-melodic sequence.

§34 It remains to summarize and evaluate the foregoing experimental data.

The facts which stand out with most prominence are, first the correlation between the beginning of a tonal sequence and a drop in rate of tapping; second, the correlation, nearly as close, between the conclusion of a tonal sequence and an increase in rate in case the observer knows in advance how many tones are to be expected; third, the retardation of rate at the end of a two-tone sequence when the observer has been led to expect three tones, the sequence being one which under the usual conditions produced acceleration instead of retardation of rate; fourth, retardations at the close are much more frequently encountered among those two-tone intervals which

are judged to be "unrelated", incoherent or decidedly "incomplete," than among intervals judged to be melodious, coherent or characterized by finality; (*vid.*, especially, descending *vs.* ascending fourth, ascending *vs.* descending fifth, minor sixth *vs.* major third); fifth, the return to a first tone is felt as giving unity to a three-tone group, and retardations at the close are not often met with, no matter how unrelated and foreign the middle tone may have been; sixth, longer sequences of tones, the pitch relations of whose elements give to them opposite characters as regards internal coherence and finality, produce opposite effects upon the rate of tapping.

In an examination of our data, these six points come to view. The attempts to apply our hypothesis in detail to some of the results must be considered, however, simply as indications toward a possible development of the method into an analytic tool of much usefulness, rather than as bringing forward further positive evidence on the question of the motor aspects of the perception of a melody.

PART IV.

SUGGESTIONS TOWARD A MOTOR THEORY OF MELODY.

Such evidence of the interconnection between muscular activity and melody experience as has been here adduced is too slender to serve as the support of an elaborate and detailed theory. But the broad lines along which a motor theory of melody must some day be worked out may be with propriety suggested here, as harmonizing with the experimental facts in so far as they are available.

§35. Every melody, like every other experience which is a 'whole,' must have, in Aristotelian phrase, "a beginning, a middle and an end." A motor theory of melody finds the 'beginning' in the upsetting of established muscular tensions which the onset of the tonal sequence involves.

The 'middle' includes the taking of the proper 'attitude,' the organization of a set of incipient responses, and then as the tonal sequence proceeds, the making of these responses explicit and overt in the acts of responding to the successive tones. Each tone demands a specific act of adjustment for which a general and also a more or less specific preparation has already been made, and each contributes in turn to the further more definite organization of the total attitude. If a tone appears which is of such a pitch that an entirely new adjustment is necessary, that tone is unrelated: unity is destroyed; the succession of tones is not a melody. But if the new tone is so related to its predecessors that it institutes a response which is in part a continuation of the act already in progress, the unity is preserved.

The 'end' comes only with the arrival of a phase of the complex ongoing activities in which the balanced tensions can merge into each other and harmoniously resolve their opposing strains. This becomes possible when a sufficiently defi-

nite set of expectations has been aroused and then satisfied. Here we find a reason why a close on the tonic has to be 'prepared for,' in musical phraseology, by a 'leading tone' not in the tonic chord. The expectations, the muscular strains and tensions, must be developed to a certain degree of definiteness of organization before a return to the tonic can serve as the cue for a general 'resolution.' '*Lösung*' describes the close of the motor process somewhat better than its English equivalent, relaxation. A single muscle can relax. But this process of muscular *Lösung* which marks the end of a melodic phrase, a spoken sentence, or a rhythmical period, is more than mere relaxation; it is an organized, balanced muscular "resolution," to borrow a very apt technical term from the musicians.

Of some such 'beginning' and of some such 'end,' even so crude and apparently remote a line of experimental attack as the one we have used, has furnished an indication. In order to learn about the nature of the 'middle' muscular processes a more refined way of approach to the delicately complex mechanism of the melody experience must be devised. One would like best of all to record the tensions of the laryngeal muscles when no sound is being emitted. Here doubtless is one of the centers, with many persons at least, of those activities by means of which a series of separate musical sounds is bound together into the unified experience we call a melody. Already some few significant facts have been accumulated regarding vocal tensions during auditory stimulation. Seashore and Cameron have independently demonstrated that a vocal tone sung against an auditory distraction tends to vary toward a pitch which is consonant with the distracting tone.[1]

Is this muscular process whose arousal and subsidence give shape and unity to a melody, a rhythm? It certainly has many of the earmarks of a rhythm,—its motor mechanism, its relaxation following tension, its conscious aspect describable as a satisfaction of expectation—all these would lead

[1] E. H. Cameron. "Tonal Reactions." *Psych. Rev. Mono. Supplements.* 1907, *8*, 287.

one to call it a sort of macro-rhythm, a giant process similar in its essential nature to a rhythm in the usual sense. But there are fundamental objections to such an identification, chief of which are (1) that a rhythm involves *repeatedly recurrent* stresses, with recognition of similarities, as this 'ground-swell' muscular process does not, and (2) that a certain regularity, with possible variations between well-defined limits only, is essential to rhythms. The two phenomena, although both motor at basis, must not be confused.

The experimental study of rhythm has, however, disclosed a motor phenomenon essentially like the large, basic motor activity underlying a melodic unity. I refer to the particular sort of muscular tension-relaxation process which Stetson[1] found to be essential to the unity of a group of rhythmic elements felt to constitute a verse, or a rhythmic phrase.

Using a modification of the principle of the phonographic recorder, Stetson made records of spoken verse, and measured with microscope and micrometer the duration and the relative intensity of the separate syllables.

In unrhymed stanzas the duration of the verse pause was found to vary widely, but it was invariably longer than the foot pause. The typical dynamic shading of the verse was found to be of the crescendo- diminuendo form. The introduction of rhyme often shifted the climax of the crescendo to the final foot by increasing the intensity of the rhymed syllable. Although as great a verse pause was found to be possible with rhyme as without it, the presence of rhyme tended to shorten the verse pause, to bring the verse to a close more rapidly.

Within the verse the general form of the syllable as it appears in the mass of closely written vibrations often varies, but nearly always shows a square end. Several very common shapes are noticed and appear in the record as 'truncated cones,' 'boxes' and 'truncated spindles.' . . ..One syllable form has an especial interest, because of its bearing on the problem of 'finality' feeling at the close of the verse. At the close of each verse, whether with or without rhyme, the syllable

[1]R. H. Stetson. Rhythm and Rhyme. *Harvard Psych. Studies*, Vol. l. *Psych. Rev. Mono. Suppl.* 1902, 4, 413.

form is always a 'cone.' Of about 600 verses measured not more than 15 are exceptions to this rule. . . . .
The form very rarely occurs within the verse, and when it does it is usually before some caesura, or under unusual conditions.

This 'cone' form of the closing syllable of the verse indicates a falling of the intensity of the voice. It is often, though not always, associated with a fall in the pitch, showing relaxation of the vocal cords. It seems to be an indication of the dying out of the intensity factor, a sinking of the tension, at the close of the verse. In the case of unrhymed verses, with long verse pause, the cone is often very much elongated, and it is quite impossible to say where the sound ceases.[1]

It will not be necessary to treat here of those portions of the motor theory of rhythm which explain, as the central, or "mental activity" theories have failed to do, the peculiar nature of the various sorts of unit groups.[2] We shall briefly sketch only so much of the theory as is requisite to explain the larger groupings such as the phrase, the verse, the period.

Stetson's theory of rhythm assumes a movement cycle involving the activity of two opposing sets of muscles. The varying tension between these muscle sets as beat follows beat never entirely disappears until the close is reached.

The continuity of the rhythmic series, whereby all the beats of a period seem to belong to a single whole, is due to the continuity of the muscle sensations involved and the continuous feeling of slight tension between the positive and negative muscle sets; nowhere within the period does the feeling of strain die out.

But at the close of the period we have a pause which is demonstrably not a function of any of the intervals of the period. During this pause the tension between the two sets 'dies out,' and we have a feeling of finality. This gradual dying out of the tension is clearly seen in the constant appearance of the cone-shaped final syllable at the end of each nonsense verse.

The period composed of a number of unit groups (the verse, in nonsense syllables) has a general form which suggests strongly that it has

[1] *L. c.*, 447.

[2] For a determination and explanation of these peculiarities, such as the closer proximity of the unaccented to the accented beat in the iambic as contrasted with the trochaic foot, etc., *cf.*, Stetson, "A Motor Theory of Rhythm and Discrete Succession," *Psych. Rev.* 1905, *12*, 293 ff.

the unity of a single coördinated movement. There is no more reason for assuming a transcendental mental activity in the case of a rhythmic period than in the case of a single act which appears in consciousness as a unity. . . . . At some point in the period there is a definite climax, a chief accent; the movement 'rises' to that point and then falls off. This is strikingly seen in nonsense verses spoken with a heavy accent within the verse. The accent does not stand out from a dead level, but the verse culminates at that point.[1]

As a result of his previous study of perceived as opposed to produced rhythms and especially the effects of rhyme and of wide variations of tempo,—'lags,'—introduced into different portions of the verse and of the stanza, Stetson was led to the conclusion that

there is some definite process at the end of the verse which marks the close of the verse and which takes more time in the case of blank verse than in the case of rhymed verse. If we conceive the end of the verse as a point where a dying out of the tension occurs, we may imagine that the rhyme brings an emphasis, and becomes a qualitative signal for this release. The slight increase of intensity on the rhyme contributes to the breaking up of the coördination, and at the same time exhausts and satisfies the feeling of tension which the verse embodies . . . . A qualitative change may be supposed to produce the effect more rapidly than the simple dying out of the tensions, which occurs in blank verse without a differentiated end accent.[2]

This finality effect which rhyme augments is entirely analogous with the finality phenomenon in melody. We have seen that in three-tone sequences mere return to the original pitch may furnish the qualitative signal for the muscular 'resolution.' If the final tone is not merely a repetition of the initial tone, but has also the characteristics of a 'tonic,' the completion of the finality process is much more definitely assured. A third cause which sometimes operates to produce the same effect is the mere satisfaction of expectation. If one hears a certain irregular series of pitches, "related" or "unrelated," often enough so that the final tone can be recognized as such, one comes to feel that the group has a certain sort of

[1] *Rhythm and Rhyme*, 455.

[2] *L. c.* 425.

unity even though there is neither a return to a starting point nor an end on the tonic. The same holds true, to a certain extent, with reference to an unfamiliar succession of tones whose number is known in advance. If the observer is told to expect four tones, a motor disposition or attitude is established which constitutes a preparedness to react to four tones, and if only three tones are heard, the finality effect may fail to appear, although the third and final tone is at once a tonic and a return to the pitch of the initial tone of the sequence.

In each of these types of melodic finality, the closing tone institutes a response which is not wholly a new reaction but which is, on the contrary, the completion of an act already in progress. The feeling of finality arises only when the completion of the act issues in a muscular relaxation which is a dying out of balanced tensions. The facts regarding those finality effects which are due to the falling inflection also coincide with such a view. Rise in pitch is not merely a result of increased tension of the vocal apparatus: it likewise produces increased muscular tension in the hearer. A falling inflection at the close consequently serves to hasten the relaxation process which marks the completion of the melody.

Finally, a motor theory of melody makes possible an unambiguous statement of the nature of melodic "relationship." Two or more tones are felt to be "related" when there is community of organized response. "Unrelated" pitches fall apart because each demands its own separate attentive act of adjustment; but with "related" tones the attitude which appears as a response to the first is a preparation for the response to the second and is completed, not destroyed, by that response. The feeling of "relationship" is the feeling that arises when the tones elicit reactions which are in some measure common. When, on the other hand, the first tone calls up one set of associates and establishes a certain attitude or organization of incipient tendencies, while the second tone tends to call up a set of associates and establish an attitude which is at variance with the first, there can be no adequacy of coördinated response and the feeling of "relationship" is prevented from arising.

The origin of these well-articulated responses which generate the feelings of "relationship" is not to be sought in a single source. The operation of two main forces must be distinguished—one of them sensory, the other associative. The first of these, the phenomenon of consonance, is native and doubtless has its basis in the relatively simple action of the sensory apparatus in responding to auditory stimuli which are more or less similar—are, indeed, in a measure identical. But although the basis for consonance inheres in the inborn structure of the nervous system and the acoustical properties of vibrating bodies, nevertheless it is a commonplace of musical history and observation that these same native tendencies are subject to tremendous modification in the course of experience. One race, one age hears as consonant intervals which another age or race has never learned to tolerate; and within the history of individuals it is easily observable that consonance and dissonance are merely relative terms whose denotation shifts with growing experience. Moreover the whole complex group of phenomena we call tonality bears witness to the power of association to amplify and organize these native feelings.

But the associative factor or the factor of experience is directly efficient in determining what tones shall be felt as "related," quite apart from any effects which it has upon judgments of consonance. Mere custom, mere habituation to a certain succession of pitches results in a facility of recognition and response which is capable of generating these feelings of "relationship." The same kind of coördinated reaction is instituted and this makes possible the same resultant feeling as that brought about by response to two successive consonant tones. The "relationship" is in both instances traceable to the motor phase of the process.

The unity, then, which marks the difference between a mere succession of discrete tonal stimuli and a melody, arises not from the tones themselves: it is contributed by act of the listener. When tone follows tone in such a manner that the hearer can react adequately to each, when the response to the successive members of the series is not a series of separate

or conflicting acts but rather in each instance only a continuation or further elaboration of an act already going forward, then the tones are not felt as discrete, separate, independent, but as "related" to each other. And when, finally, the series of tones comes to such a close that what has been a continuous act of response is also brought to definite completion, the balanced muscular "resolution" gives rise to the feeling of finality, and the series is recognized as a unity, a whole, a melody.

[Reprinted from THE PSYCHOLOGICAL REVIEW, Vol. XVII., No. 3, May, 1910.]

THE PENDULAR WHIPLASH ILLUSION.[1]

BY ALGERNON S. FORD.

Professor Dodge, in an article entitled 'The Participation of the Eye Movements in the Visual Perception of Motion,'[2] attaches considerable importance to a certain phenomenon which he terms the 'whiplash illusion.' This illusion is obtained by fixating one of two moving lights which are attached to the two arms of a counter-balanced pendulum. The fixated light is perceived centrally, the non-fixated, peripherally. "When the point fixated approaches its extreme position in each oscillation, it seems to rest for an appreciable interval, while the other point seems to continue moving as though the two were connected by an elastic rod, which regularly gave the unfixated point a considerable additional oscillation after the fixated point had been arrested at the end of each swing. The illusion is persistent and striking, and is capable of only one explanation. It occurs at that part of the pursuit movement which photographic registration shows to be practically free from corrective movements. The fact that the point whose image remains motionless on the retina during an unbroken pursuit movement seems to stand still, while the other point, which is in reality moving no faster than its fixated companion, seems to make a little gratuitous whiplash excursion, serves at once to show the utter inability of the pursuit movement either to subserve the perception of motion of the fixated point or to correct the exaggerated data from the displacement of the retinal image of the non-fixated point."[3] Dodge has given a good description of the whiplash illusion, and he believes that this illusion sustains his theory that pursuit movements do not subserve the perception of motion. No positive theory, explanatory of the illusion, is found in the article in question, and it is by no means a certainty that

[1] From the Harvard Psychological Laboratory.

[2] Dodge, R., PSYCHOLOGICAL REVIEW, 1904, Vol. XI., pp. 1–14.

[3] *Op. cit.*, p. 14.

Dodge's claim, that the illusion is 'capable of only one explanation,' will hold after further experimental data have been submitted.

Dr. Harvey Carr, in an article[1] entitled 'The Pendular Whiplash Illusion of Motion,' takes a view opposed to that of Dodge, and suggests two possible explanations of this illusion. His first theory, which we shall for convenience term the *limen* theory, is given below: "As the pendulum approaches the end of its swing, the rate of movement gradually decreases to zero. Consequently, for some definite portion of the end of its swing, its rate would be below the eye movement limen, but still above the retinal limen of perceptibility. In other words, the retinally perceived light would be seen moving for an appreciable time after the fixated light had apparently stopped. Hence the gratuitous whiplash excursion is evident."[2] This positive assumption by Carr is supported by historical opinion. It has been commonly held that eye movements could mediate visual motion for the greater magnitudes and velocities, and that the limen of perception here is higher than the limen for retinal displacement.[3]

Carr gives most attention to his second or *positive after-image streak* theory. When the fixated light is followed properly, no positive after-image streak is appreciable, at least for the last portion of the swing. Unless the non-fixated light be very weak in intensity, it will leave an appreciable positive after-image streak. "The eye moves in a direction opposite to this latter light [non-fixated] and consequently the rapidity of its retinal displacement equals that of a light, perceived by a stationary eye, moving at a rate equal to the combined velocities of the two lights used in the pendulum test. Other things being equal, the length of the after-image streak varies directly with the rapidity of the retinal displacement. Thus a very pronounced length of the positive streak results in the test. This light, with its positive after-image, is viewed peripherally and hence is seen indistinctly and *en masse;* without conscious effort on the part of the observer, it appears as an elongated light with

[1] Carr, H., Psychological Review, 1907, Vol. XIV., pp. 169–180.

[2] *Op. cit.*, p. 171.

[3] Aubert, H., 'Die Bewegungsempfindung,' *Pflüger's Archiv*, 1886, Bd. 39, S. 347–70; 1887, Bd. 40, S. 459–480.

no very decided contour, nor sharply discriminated parts."[1] As the pendulum approaches the end of its swing, this elongated mass of light rapidly contracts in length at its rear end because both the eye and the non-fixated light come to rest so that the after-image streak is no longer generated, and the older (here the rear) portion of the after-image fades the sooner. "If the positive streak is six inches long when the pendulum is one inch from the end of its swing, and this streak has time to disappear while the pendulum is moving and returning over this final inch of its arc, it is evident that the total mass of light will have contracted at its rear end from six inches to one inch in length. These values are of course merely illustrative. . . . Consequently, the whole mass of light will appear to be moving on, after the pendulum has really stopped. The observed extra movement is thus a purely illusory one."[2] It might be stated here that the author of the after-image theory does not assert this factor to be solely responsible for the illusion.[3]

The following conditions obtained in Carr's experiments. The lower arm of the pendulum was 78 cm. in length, the upper arm, slightly shorter. The lower arm was made to swing through an arc of 160 cm. Two seconds were consumed in a forward and return movement (complete swing). The observer was 230 cm. away from the lights. Two very small incandescent lights of low intensity were used. The tests were performed at night in a dark room.

The following conditions obtain in the present writer's experiments. Two miniature incandescent lights are attached to a counterbalanced pendulum. These lights are encased in wooden cups which have on their fronts circular openings of .5 inch diameter. The entire mechanism is placed behind a plate of ground-glass, the lights being brought very close to this glass. By careful regulation of the electric current and the use of small obstructive discs of milk-white celluloid, the intensity of the lights can be reduced to any degree. The lights are adjustable to any positions on the pendulum. Whenever it is desired to have a light at the axis, a third enclosed lamp is sus-

[1] Carr, *op. cit.*, pp. 171–172.
[2] *Ibid.*, p. 172.
[3] *Ibid*, p. 178.

pended before the glass plate, and reduced to the required intensity. The best illusions are obtained by the writer when the lights are at short distances from the axis, *i. e.*, one inch to ten inches. The observer is stationed at a distance of seven feet from the lights. The tests are conducted in a dark room.

The pendular whiplash illusion is complex, its factors of illusion being: (1) the apparent unequal distances traversed by the two lights, (2) the apparent unequal speed of the two lights, and (3) the apparent movement of one light temporally longer than the opposite movement of the other.

I. *The Apparent Unequal Distances Traversed by the Two Lights.* — Dodge says: "If the distance of both from the axis was equal, both would move through equal distances in the same time. The one fixated however always appeared to move much less than the one seen peripherally. It was found that if the two were to appear to move through equal arcs, the pursued must actually move through about three times the arc of the unpursued. This of course could be accurately measured by the relative distances of the two points from the axis."[1] As regards his investigation of the above point, Carr has this to say: "My observers did not confirm these results as to the apparent lengths of movement. In fact, they gave judgments of equality of movement only when the two arcs were practically equal in length."[2]

My subjects gave judgments to the effect that the non-fixated light apparently moves a much greater distance than the one fixated. The difference of apparent distances traversed by these two lights is clearly shown in a modification of the experiment where a piece of cardboard, of sufficient size to allow the non-fixated light to be seen only at the very beginning and the end of its sweep, was placed over this light's path, this eliminating the after-image streak which might be thought responsible for the greater distance apparently moved through. The non-fixated light still appeared to travel much farther than the fixated. No exact measurements were made in the study of this factor (distance). It is undoubtedly significant that the non-fixated

[1] Dodge, R., *op. cit.*, pp. 13-14.

[2] Carr, *op. cit.*, p. 173.

light moves across the retina twice the angle that the eye-ball traverses in its socket in fixating the other moving light.

The illusion in the case of the modified experiment above was striking, though the positive after-image streak was eliminated as a factor in its determination.

For short arm lengths, there is a more marked difference between the apparent lengths of the arcs (described by the two lights) than for greater lengths, except where the opaque disc is used as already described.

II. *The Apparent Unequal Speed of the Two Lights.* — It seems natural that the non-fixated light should appear to move faster than the fixated, since it appears to move farther. It is the consensus of opinion of my subjects that such a difference is pronounced, yet no exact experimental data were secured to sustain such a view.

III. *The Apparent Movement of One Light Temporally Longer than the Opposite Movement of the Other.* — This factor is the whiplash illusion proper. The references and theories cited in the first part of this article, bear directly upon this factor.

There is an appreciable difference in the quality of the illusion for great, and very short lengths (of the pendulum arms). In case of a ten-inch length of the arms, the pendulum appears to bend at or near the axis, while in one- or two-inch lengths the pendulum appears rigid, the non-fixated light depending from the fixated as a point of support.

When very short lengths are used, so that the lights move slowly, a few of my subjects observed practically no motion of the fixated light.

It makes no difference in the illusion itself whether the lower or upper light be the one fixated. The illusion is equally appreciable for horizontal, vertical, and oblique oscillations. In the latter two cases however, greater difficulty of fixation is experienced. This is probably due to the comparative difficulty of vertical and oblique eye movements.

An interesting variation of the experiment is made by placing one of the two lights at the axis. When the moving light is fixated, an illusory motion of the stationary light is observed.

In order to establish a pendulum arm-length (within reasonable limits) at which the illusion is most pronounced, the contents of Table I. (which follows) were obtained. The several degrees of illusion are reported as Weak (W), Medium (M), and Strong (S). The absence of illusion is expressed by the letter N. Intermediate degrees are indicated by the plus and minus signs. For the purpose of numerical comparison, N, W–, W, W+, M –, M, M +, S –, S, S +, and S ‡, are represented respectively by the arbitrary values 0, 1, 2, 3, 4, 5, 6, 7, 8, 9 and 10.

TABLE I.

UPPER LIGHT FIXATED AND ATTENDED TO.

Name.	10″—10″			8″—8″			6″—6″			4″—4″			2″—2″			
H	S	S	S	S–	S–	S	S	M	S	M	M	M	S	S	S	→
R	S–	S–	S–	M+	S–	S–	S	S	S	S	S	S–	S+	S+	S+	→
F	S	S–	S	M–	M	M+	S–	S–	S	M+	S–	S–	S–	S	S+	←
R′	W	M	M	M	S	M	S	S	S	S+	S+	S+	S‡	S‡	S‡	→
N	M+	M	M+	W–	M+	S	M+	S	M+	S	S	S	S+	S+	S+	→
M	M	M	M	M	M	M	S	S	S	M	M	M+	S	S+	S+	←
M′	S+	S+	S+	S–	S–	S–	M	S–	S–	N	M–	M–	N	W	N	←
N′	W	M	S	M	M	S	W	S	S+	M+	M	S–	M	M	M	←
Num. Equiv.	154			144			173			151			175			
Av. Equiv.	6.4+			6.0			7.2+			6.3+			7.3+			

NOTE:—(1) Figures at the top show the distances of the two lights from the axis of the pendulum. (2) Arrows indicate direction of procedure with the several subjects. (3) Each subject gave three judgments for each position of the lights. (4) Oscillations are horizontal; upper light fixated. These conditions prevail throughout the experimentation except for Table V when a third, or central, light is fixated.

From the, to be sure, somewhat irregular results of Table I., it would seem that the shorter lengths of the pendulum arms favor the illusion.

A modification of the regular experiment, made by placing a grating over the fixated light, gave very interesting results. In case the grating was placed over the fixated light while the pendulum was in motion and the illusion appreciable, a diminution of the illusion followed instantly. The grating adds the factor of image displacement on the retina to the usual perception of the motion (whatever this consists in) of the fixated light. This gives evidence in support of Carr's *limen* theory.

Table II.

Grating Used Over Fixated Light.

Name.	10″—8″			10″—7″			10″—6″			10″—5″			10″—4″			10″—3″			10″—2″			10″—1′		
H	W	W	W	W—	W—	W—	W	W	M	W—	M	W	N	N	N	N	W—	W—	N	N	S—	N	N	W—
R	S—	M+	S	M+	M+	M+	W+	W+	M+	W	M+	S—	W+	M—	M+	W	M—	M—	M—	M—	S—	M+	M+	S
F	S—	S—	S	M+	S—	S—	S	M+	M+	M	M	M	M	S	M	W	S	M	W	W	W	W—	W—	N
N	M	M+	M+	W	W	M+	W	W	M—	W	W	M	M	M—	S—	S—	W	M—	W	M	M	M	S	M+
No. Eq.	69			51			49			47			47			40			40			41		
Av. Eq.	5.75			4.25			4.08+			3.91+			3.91+			3.33+			3.33+			3.41+		

Note: (1). Subjects *R′*, *M*, *M′* and *N′* were not available for this experiment.

By a comparison of Tables I. (or III.) and II. it is seen that the grating diminishes the illusion considerably. Out of ninety-six judgments in Table II. there are ten where the illusion was absent altogether.

Carr states (p. 177) that the illusion 'is conditioned by the direction of the attention.' In obtaining Table I. the importance of this statement was frequently realized. In preliminary tests seven out of eight subjects reported the illusion to be better when the attention was directed to the fixated light than when directed to the peripherally perceived light. It seems that when the fixated light is attended the non-fixated light is neglected, and so is permitted to execute its movement without in any way being checked, in other words, it is allowed to go free, whereas, in the event that the non-fixated light is attended, it is kept vividly in consciousness, and therefore we seem to *keep pace* with it and to perceive its motion as being perfectly normal. This led to the formation of Tables III. and IV., the results of the former being obtained by carefully attending the fixated light, those of the latter, by attending the non-fixated light.

The values of *R*'s judgments in Tables I. and III. are respectively, 143 and 132 points. This shows a situation contrary to his introspection that it is when the non-fixated and not the fixated light is attended that the illusion is greater. With the exception of *R*, it was reported by the observers in the experiments of Table IV. that whenever the illusion was perceived, it was distinctly felt at the time that the attention had wandered *back* from the non-fixated to the fixated light or to some intermediate point.

The decrease in the degree of the illusion (in Table III.), as indicated by the numerical equivalents, is very likely due to the distracting influence of the non-fixated light upon the attention as the lights become closer together. In other words, it appears that the attention, at short distances of the lights, is inclined to pass to the non-fixated light, thereby diminishing the value of the illusion.

We observe in Table IV. a general decrease toward the smaller distances. This appears reasonable, *for the non-fixated light can be more easily attended at the shorter distances.*

TABLE III.

ATTENTION ON THE FIXATED LIGHT.

Name.	10''—8''			10''—7''			10''—6'			10''—5''			10'—4'			10''—3''			10''—2''			10''—1''		
H	M	M	M	S—	M+	M	M	M	M	M+	M+	M	M+	S—	M	M+	W	M	S—	M	M+	W	W	$\overrightarrow{\mathrm{W}}$
R	S—	S—	S—	S—	S	S—	M+	S—	M+	M—	M—	M+	S	S—	S—	M+	M+	M	M—	M	M+	M—	M—	$\overrightarrow{\mathrm{M}}$
F	S$^{+}_{+}$	S$^{+}_{+}$	S+	S+	S+	S	S+	S+	S+	S+	S	S+	M	S	M+	S—	S—	M+	M+	M+	M+	W+	M—	$\overleftarrow{\mathrm{M}}$
N	S	S	S	S	M+	S	M+	S	S	M	M+	S	M+	M	M+	M	M+	S	M	M+	M+	W	W	$\overleftarrow{\mathrm{M}}$
N'	M	M	M	M	M+	M+	S—	M	S	W	M	W	M	M+	S+	S+	S+	S+	S	W	M	W	M	$\overrightarrow{\mathrm{M}}$+
No. Eq.	104			105			103			85			96			96			82			53		
Av. Eq.	6.9+			7.0			6.9+			5.7+			6.4			6.4			5.4+			3.5+		

TABLE IV.

ATTENTION ON THE NON-FIXATED LIGHT.

Name.	10″–8″			10″–7″			10″–6″			10″–5″			10″–4″			10″–3″			10″–2″			10″–1″		
H	W—	N	N	N	N	N	N	W—	N	N	N	N	N	N	N	N	N	N	N	N	W—	N	N	↓ N
R	S—	S—	M+	M+	M+	S—	M	M—	S—	M	M	M	M+	M+	S—	M	M+	W+	M	M	M	M	M	↓ M—
F	W	N	W+	M	W	N	W	M+	M+	W—	W—	N	W—	N	W—	W—	W—	W—	N	N	M	N	M	↑ N
N	W	N	W+	N	W	N	N	W	N	W	W	N	N	N	W	W	N	N	N	N	W	N	N	↓ N
M′	W	W	W	W	W	N	N	N	N	W	N	W	N	N	W	W	N	W	W	N	N	N	N	↓ W—
No. Eq.	37			32			33			20			25			23			25			20		
Av. Eq.	2.5+			2.1+			2.2			1.3+			1.7+			1.5+			1.7+			1.3		

Note: (1) The light *ten* inches distant from the axis is the one fixated during experimentation for Tables III. and IV. (2) *R′* *M*, and *M′* were not available as subjects for Table III.; *R′*, *M*, and *N′* not available for Table IV.

In order further to test the influence of direction of the attention on the illusion, the following variation of the experiment was made. Three lights were used, two as before and the third made stationary at the axis of the pendulum. At all times this *central* light was to be fixated. After-image streaks were eliminated by reducing the intensity of the lights. The observer was given a position seven feet from the lights. His head was held stationary by clamps. A small light was thrown from the side on one eye, the other eye being covered.[1] A reading telescope was trained upon the eye so that the slightest motion could be detected. The two peripheral lights were each eight inches from the axis. The observer was instructed to direct his attention to *one* of the peripheral lights, his fixation being on the central light however.[2] Under these conditions the non-attended light regularly appears to move for a longer time than the attended light. The observer then reported each time that the illusion appeared. Fifteen judgments of illusion were taken for fixation of both lower and upper lights. The illusion is readily perceptible, but appears irregularly, depending upon the accuracy of the attention's direction. By the use

Table V.

Three Lights: Central Being Stationary and Fixated.

Name.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
H	o	o	o	o	o	o	o	o	o	o	m	o	o	o	o
	o	o	o	o	o	o	o	o	o	o	o	o	o	o	o
R	o	o	o	o	o	o	o	o	o	o	o	m	o	o	o
	o	o	o	o	m	o	o	o	o	o	o	o	o	o	o
F	o	o	o	m	o	o	o	o	o	o	o	o	o	o	o
	o	o	m	o	o	o	o	o	o	o	o	m	o	m	o
N	o	o	o	m	o	o	m	o	o	o	o	o	o	o	m
	o	m	o	o	o	o	o	o	m	o	o	o	o	o	o

Note: (1) o = no eye-movement; m = eye-movement.

(2) For every judgment (whether o or m) there was an illusion.

(3) Each of the two pendulum lights 8 inches from the axis.

(4) Upper row of judgments for each subject is given for case where the upper light is attended, the illusion being of the lower light; the lower row of judgments, when the lower light is attended, the illusion being of the upper light.

(5) *R′*, *M*, *M′* and *N′* were not available as subjects for this experiment.

[1] In all previous experiments, both eyes were used.

[2] As to the possibility of such an adjustment, cf. Helmholtz's *Physiol. Optik*, 2te Auflage, 1896, S. 605.

of the telescope, the experimenter was able to note any eye-movement at or immediately preceding the instant of report. Out of one hundred and twenty judgments as seen in Table V., there were only twelve where the experimenter was at all uncertain as to the immobility of the eye.

This last experiment shows conclusively that the illusion is to a large degree influenced by the direction of the attention. The effect produced by the attended light is perceived *prior* to that of the non-attended light. When the attended light has reached the end of its swing, the non-attended light is of course neglected and slow to enter consciousness. When the non-attended light appears to be reaching the end of its swing, the attended is on its way back, having been transmitted to consciousness before the former. The apparent bending of the pendulum occurs at this time. *It is evident that the pendular whiplash illusion is in part a special case of the law of prior entry of attention.*[1]

Of the three factors of illusion, that of the whiplash *excursion* has received most attention at the hands of the investigators, but this in no wise should indicate that the factors of apparent distance and velocity of the lights are unimportant. Dodge has failed to give a theory accounting for the illusion. He uses it (the illusion) as an instrument in his attempt to prove that the pursuit movement is unable 'to subserve the perception of motion.' Carr offers the *limen* and *after-image* theories both separately and conjointly in explanation of the illusion. The *limen* theory, in addition to claims already advanced for it, is supported by Table II. of this article. There is no incompatibility between the *limen* theory and the *attention* factor. The positive after-image streak is possibly a factor of some importance, but it is well to remember that the illusion can be produced without the influence of this streak, as in the case where an opaque disc was placed over the path of the non-fixated light. The *after-image* theory is further weakened by the fact that when the after-image was eliminated (as above when a piece of cardboard was placed over the non-fixated light's path) the

[1] See Titchener, *The Psychology of Feeling and Attention*, p. 251, for the law of prior entry.

distance (apparent) traversed by the non-fixated light was *greater* than before. The results of Tables III., IV. and V. sustain the factor of *attention*.

From the foregoing investigation we seem bound to conclude that the pendular whiplash illusion is a complex phenomenon depending on one or all, according to the conditions, of the following factors: (1) the fading of after-image streaks at their older ends; (2) the lower threshold of movement perception by means of displacement of the retinal image, as compared with the threshold of movements as perceived by movements of the eyes; (3) the law of prior entry of attention.

377/

Ae.3.

ON OCULAR NYSTAGMUS AND THE LOCALIZATION OF SENSORY DATA DURING DIZZINESS.

BY PROFESSOR EDWIN B. HOLT,
Harvard University.

If a person sitting with his eyes closed and his head in an upright position is slowly set in rotation about a vertical axis lying within or near his head, he meets with a considerable variety of sensory experience. All these sensory data contribute more or less toward the perception of motion, but in different ways. And these data fall into three general groups which it takes no extremely subtle introspection to distinguish.

1. Firstly, one distinguishes a group of sensations which proceed from extra-peripheral stimuli: currents of air are felt by their impact or their temperature, on any uncovered surfaces of skin; any source of light which can be dimly perceived through the eyelids or eye-bandages, will be seen intermittently as with each rotation the face is brought opposite the light; any source of sound will be heard alternately loud and faint as with each rotation an ear is twice presented to it. The air currents would of themselves give no clue to the movement of the person's own body, and they are ordinarily felt as somewhat irrelevant data, which hardly even tend to fuse with the other sensations of motion. The visual and the (doubly rapid) auditory intermittences become, with increasing speed of rotation, rhythms of which the spatial or temporal significations are subject to considerable individual differences. Thus for my own introspection the visual intermittence becomes a temporal rhythm, while the auditory sensations become a hoop of sound lying horizontally about the head as a center, and having two spots of maximum loudness opposite each other, and two of minimum loudness midway between them. I have sometimes, though seldom, had from the visual intermittence a comparable hoop of light. In any case the person is aware of a relative motion

between his body and the hoop of sound, or light, but which of these is at rest and which in rotation is so far ambiguous and is determined by the factors given in group 3. Both the visual and the auditory phenomena are readily isolated in introspection, and both are felt to be distinctly 'secondary criteria' of motion.

The three sorts of sensation so far mentioned may all be eliminated by fairly simple precautions (though they are also readily ignored by the observer), but another kind of sensation is neither so readily elimated, nor ignored, nor distinguished from the sensations of group 2. This kind comprises the tactual sensations mainly of the hands, back, thighs and soles of the feet, which vary with the inertia of the body and with the centrifugal moment induced by the rotation. These sensations, while less clearly a secondary criterion of motion, while more intimately associated, that is, with the sensations of group 2 and even of group 3, can still after some practice be distinguished as tactual sensations of varying strength.

2. Secondly, there are the sensations from proprio-ceptive organs (Sherrington, '06, p. 130) in joints, muscles and other tissues, which are stimulated (similarly to the last-named class of group 1) by the inertia of the trunk, limbs, internal organs, and even perhaps of the blood, and by their centrifugal moment. Sensations supposed to be stimulated in this way in the cerebellar mass or its sensitive coatings were originally adduced by Purkinje (Aubert, '88, S. 119–120) to explain dizziness; and in connection with movements of translation Delage ('86, p. 623) has referred to sensations seemingly "produced by a sort of internal tidal movement in which all the liquids and such solid organs as have any mobility, participate." While such factors are hypothetical, certain sensations from proprio-ceptive organs in joints, tendons and muscles undoubtedly play a part in the perception of the motion of one's own body (Schäfer, '87; Breuer, '90, S. 204; Mach, '73, S. 127; Abels, '06, S. 382). It is difficult even after practice to distinguish introspectively these sensations from those of group 3, except when, with a high speed of rotation, they become intense, whereupon they are readily distinguishable as secondary criteria, from the true

sensations of movement of group 3. How much these proprio-ceptive stimulations when not intense contribute to the perception of movement is hard to determine. Some writers, as Abels ('06 and '07), have wished to find in them the very basis of that perception, but this is an unwarrantable view for as Mach ('74, S. 130) has said: "One can scarcely explain feelings of motion in terms of skin or muscle sensations, in view of the feelings in the head, the enormous influence of the head position, and Flourens's experiment:" and indeed Mach might have added, in view of well-nigh every fact that experiment has yielded regarding the canals and sacs of the ear (cf. also Breuer, '07). Or, as (Crum) Brown has said ('95, p. 15), "A few experiments . . . will convince any one that we have here to do with a perfectly definite sense, and not with any vague sensations caused by the inertia of the soft parts of the body." Nevertheless the proprio-ceptive sensations are of interest here, and deserve more experimental notice than they have so far had.

3. Thirdly, there are the true sensations of motion which are in some way dependent on the semicircular canals, and probably the sacs, of the ear. These present a remarkable complication of phenomena, with which we shall have chiefly to deal in the present paper. And firstly introspectively. If a person sitting with his eyes closed and head upright is rotated about a vertical axis within or near his head, and if the speed of rotation increases continuously, the person feels his body to be rotating in the direction of the actual motion, and he also generally feels objects in the space around him (by as much as he is aware of them) to be moving more or less rapidly in the opposite direction. Two things, in short, the body and the objects around it, are felt to be in relative motion.

Problem I. — What organs yield the sensation of rotation? I believe that it has not so far been noticed that unless the rotation is very rapid, the *direction of the attention* is able to determine which of these shall be felt to be the more involved in motion and which to be almost or quite at rest. If, namely, the person 'directs his attention' to his own person, *this* will seem to be in rapid motion while then the environment may

seem to be quite at rest: but if the attention is directed to the environment sweeping by, if, that is, the sensations of groups 1 and 2 occupy the focus of attention, the objects about the person will seem to whirl rapidly to the rear while his own body will seem to be nearly or quite at rest. I find also in this case a faint suggestion in consciousness of a space far behind these dimly presented objects, which is, like my body, at rest. When the rotation is rapid, however, it is much more difficult and often impossible to achieve such a 'setting' of the attention. Also, as we shall later see, some individual differences are to be expected in this field. This influence of the direction of the attention on the perception of motion during rotation is doubtless analogous with the effect mentioned by Hering of the same factor, on the apparent position of objects.

Problem II. — What is it which is involved in what we introspectively call 'setting the attention,' which in the case of rotation can shift the appearance of motion from one object to another?

If the rotation is long protracted at an ever-increasing rate of speed, the person becomes sick. This phase is no part of dizziness proper, and does not here concern us. The remaining introspective phenomena which interest us occur when the speed of rotation is decreasing, and after the rotation has stopped. Now when the acceleration of motion changes from positive, or zero, to negative, the person feels without appreciable latency *both* himself and the objects around him to be rotating in the contrary direction. Here, too, the direction of the attention is of influence, but here the attention is to be directed *against* the illusory motion, stemming the tide as it were, and then the movement *both* of the body and of such sensations as one has of the outer objects (group 1) is alike diminished or annulled.

Problem III. — How does setting the attention against the illusory post-rotary movement reduce the apparent motion of both the body and the enviroment?

If now the eyes are opened, the apparent rotation persists (although cf. Bárány, '06, S. 223) save that the visual field is far more prominent than before; and it is still whirling con-

trariwise to the original rotation. As Mach ('73, S. 127) has described it: "As soon as the apparatus is slowed down one has the feeling of making a contrary rotation together with the box [in which one is enclosed]. If now the box is opened, entire visual space with its contents rotates. *It is as if all visible space were turning within a second space which one believes to be motionless, although it is identified by no visible cue.* One might almost believe that there exists behind visual space another space to which the visual is always referred."

Furthermore, during the decrease or immediately after the end of the rotation, objects presented to the tactual sense are felt to be in contrary rotation, similar to that of the visual field. This tactual dizziness is far less pronounced than the visual, but is sufficiently attested by Purkinje ('20), Mach ('00, S. 100), Wundt ('02, Bd. 2, S. 586) and others. There is likewise an auditory dizziness quite analogous to the preceding, whereby sources of (continuous) sound appear, after the rotation, to rotate contrariwise. As Münsterberg and Pierce ('94, p. 475) have described it: "If after the rotation, but while the eyes were still closed, the sound was given continuously for a time, it seemed to make the illusory movement too: it remained, that is, in constant orientation with the body." These visual and auditory phenomena experienced *after* rotation are not, of course, to be confused with the 'hoops' and other phenomena of group 1 experienced *during* rotation.

The foregoing phenomena immediately suggest the following problems:

Problem IV. — Why does the body appear to reverse its motion and to rotate contrariwise when the acceleration becomes negative and after the actual rotation has ceased?

Problem V. — Why after the rotation has ceased does the visual field continue to rotate contrariwise?

Problem VI. — Why do tactual and auditory impressions likewise continue to rotate contrariwise?

While the behavoir of the visual field has since the early seventies been referred in a general way to the ocular nystagmus which is normally induced by rotation, the connection between the two is still susceptible of elucidation: and I am not aware

that any specific explanation has been offered for the tactual and auditory dizziness. It was with these problems in mind that I undertook in the fall of 1908 some experiments, on which the following discussion is partly based. It is also based in part on some experiments with dizziness previously described by me ('06), and on the literature of the subject.

I. What Organs Yield the Sensation of Rotation?

On this question a very large number of investigators have come, although in a very general way, to some appearance of agreement. In general nearly all assent to the hydrokinetic theory of Mach, Breuer and (Crum) Brown,[1] and I believe that in the main this theory is established beyond all question while some of its details will bear further examination. The experimental facts show that the semicircular canals of the ear are stimulated by circular and rotary motions, and that the utricle and saccule are (almost certainly) stimulated by motion of translation. (Some writers dispute the latter point, although, as it seems to me, on hardly sufficient grounds.) And from this the conclusion is commonly drawn, that these organs yield *directly* the sensations of rotation and of translation. The argument is —(1) sensations of motion depend on the position of the head; (2) the only receptor organs situated in the head, which are stimulated by motion, are the ampullæ and sacs of the ear; (3) therefore the sensations of motion are sensations from the ampullæ and sacs. Now clearly the two premises warrant the conclusion that — therefore the sensations of motion *result* from stimulation of the ampullæ and sacs. But these sensations need not result *directly*, as the first conclusion affirms. Nor have I so far discovered any experiments adduced explicitly to show that sensations of motion result directly rather than indirectly, from labyrinthine stimulation, although the former is commonly assumed. Bárány ('06, S. 265, S. 275–6) touches on this point and declares: "It is an error to say that excitations thereof [*i. e.*, of the canals and sacs] do not come to consciousness; *strong excitations of them do come to consciousness*, either as

[1] This theory has been admirably summarized by Nagel ('05, S. 790) and by Peters ('05).

such or in combination with [unter Mithilfe d.] the accompanying eye-movements" (eye-movement sensations?). But the ground of this affirmation is neither here nor elsewhere made clear. The only other reference to just this point, which I have found, is in Ewald ('92, S. 133): "There are not merely special physiological movements, which take place during and after rotation, but also these are accompanied by special sensations. But the relation existing between the two has never been made clear. It has been taken for granted that the abnormal movement ensues on an abnormal sensation, and is in a way its visible expression. But I do not believe that the relation is such a simple one." And in another place (S. 141) Ewald finds the movements following rotation to be 'reflex,' by which he clearly means that they are produced directly by the labyrinthine excitations *without* these latter having come to consciousness. There is, then, ground for debate in this matter.

On the other hand it has been generally granted that the nystagmic movements of the eyes are closely connected with the visual dizziness. Thus at the very outset Purkinje attributed visual dizziness to 'unconscious' eye-movements carried over [übertragen] to outer objects (Aubert, '88, S. 117). Delage, too, while deeming the labyrinth an organ of sensation in the strict sense, ascribes features of visual dizziness to movements of the eyes ('86, p. 610–11). Mach ('00, S. 98–100), Breuer ('98, S. 499) and Kreidl (*ibidem*) also attribute visual dizziness and illusions as to the vertical, to nystagmic and compensatory eye-movements; although these writers too believe in direct labyrinthine sensations. The accepted view should seem to be, then, although I do not know of an explicit statement to this effect, that movements of one's own body in rotation and translation are perceived by means of sensations coming directly from the ampullæ and sacs, while visual illusions of rotation and many of motion and position are due to reflex (and 'unconscious') eye-movements. Motion of translation of one's own body would be perceived by means of sensations from the sacs, of rotation by sensations from the semicircular canals.

Now there stands in somewhat surprising contrast to this view the experimental fact that both during and after rotation

the sensation of rotation of one's own body is instantly inhibited if the ocular nystagmus is inhibited. This observation was first made by Bárány ('06, S. 224): "*The direction of one's line of regard is also of influence on the illusory sensation of rotation. If I have nystagmus horizontalis to the right* [*i. e.*, the rapid eye jerk toward the right and slow movement toward the left], *which I can inhibit by looking toward the left, and if with my eyes closed I do look toward the left, the sensation of apparent rotation stops at once—just as the apparent motion of outer objects had* [previously and for the same reason] *stopped. If I look again to the right, the illusory motion commences again. One can observe several such disappearances and reappearances of the sensation of rotation. A considerable number of physicians was able to observe this phenomenon, which I am the first to describe, and I have had the same reports from enquiries among patients.*"

I have previously reported ('06, p. 72) that the slow phase of the ocular nystagmus can "not voluntarily be inhibited; whereas the swift movement is so far voluntary that it can be *inhibited* at pleasure. It is possible, that is, to fix the eyes on that side of the field toward which the slow movements are directed, but not on any point at the other side of the field." And this inhibition of the nystagmus always inhibited visual dizziness; but I had not at that time noticed that it also inhibits the apparent rotation of one's own body. Now conflicting statements are to be found on this point, and the most emphatic are those of Mach, who states ('74, S. 123) "that a person can have very marked subjective phenomena of rotation with demonstrably fast fixation and no eye-movements. If on the inside of the paper box described in the previous communication [the observer was inside the box, and both rotated] there is fastened a black cross on a white ground, so that when one fixates the crossing every deviation of the line of regard is betrayed by an after-image, then one observes no such after-image when dizziness starts up. One can fixate and still feel dizzy. I have also convinced myself by direct observation of the eyes of a second observer, that the eyes can remain at rest when the experiment is carried out in the way I have described."

And twenty-six years later Mach again wrote ('00, S. 101–2) to the same effect, although here it is *visual* dizziness rather than the sensation of bodily rotation which is not inhibited by voluntary fixation of the eyes. For Mach both kinds of dizziness undoubtedly persist.

It chanced that a few weeks before learning about the observations of Bárány, two other observers, Dr. Tait and Mr. Ricker, and myself noticed (accidentally, for we were then interested in visual dizziness) that voluntarily inhibiting the nystagmus does away with the sense of bodily rotation not merely after the rotation has stopped, but *during the actual rotation itself*. We were all three able repeatedly to undergo a lively passive rotation (axis of rotation vertical, head erect and over axis, eyes closed) of one to two minutes without *at any time* having the sensation of bodily rotation.

Such an inhibition of nystagmus *throughout* the experiment can be accomplished only in this way: Before the chair is set in motion (by a second person) the observer directs his fixation as far as possible in the direction *contrary* to the coming rotation, and holds his eyes in this position as long as the acceleration remains positive. With the eyes closed, as here, this requires some practice and we found that it could be facilitated by securing a fairly durable after-image on the retinæ immediately before the experiment. When the acceleration has nearly reached zero, *i. e.*, when the speed has become nearly constant the observer relaxes his fixation and lets his eyes do as they will. They wander slowly toward the primary position of regard and remain there as long as the acceleration stays at zero. No motion of the body is felt if the voluntary control of the eyes is relinquished at the right moment. As soon, now, as the motion begins to be reduced (the acceleration is negative) the eyes wander involuntarily to the other side, *i. e.*, *with* the actual rotation, and here they must voluntarily again be fixed until several seconds after the rotation has actually ceased. The experiment requires that the observer shall not actively assist to rotate himself.

If this is successfully accomplished all sensations belonging to group 3 are inhibited leaving, however, those of groups 1

and 2. The 'hoops' (group 1) continue to rotate contrariwise as long as the actual movement lasts, but no longer (although they would continue contrariwise still longer if the nystagmus were not inhibited), and the centrifugal sensations are distinct in consciousness; and yet so insignificant are these secondary criteria of motion as compared with the primary sensations thereof, that the subject feels himself to be at rest in a somewhat remote though whirling entourage. This motion of surrounding objects is far from being adequately realized, as we ascertained occasionally by opening the eyes during rotation, whereupon the sudden realization of the rapid movement (backward and contrary to the actual movement) of visible objects came as a shock. But even this does not reinstate the sense of one's body being in motion provided that the nystagmus is still inhibited. It startles one sometimes into relaxing the hold on one's chair, so that we found it to be very disagreeable and somewhat risky to open the eyes while the nystagmus (and therewith the feeling of rotation) were being suppressed *and* the rate of rotation was rapid. The three observers above mentioned were well trained in the observation of dizziness; another subject, with less training and rather easily nauseated by dizziness, underwent the rotation without a sense of being himself in motion: and two women, quite untrained in the matter of dizziness, suppressed the post-rotary feelings of bodily rotation on the first trial, by inhibiting the post-rotary nystagmus; and on second trial succeeded in feeling no motion during as well as after the rotation. In all the experiments I observed nothing which would lead to any other conclusion than that voluntary inhibition of the ocular nystagmus *directly* inhibits the sensation of the rotation of one's own body.

These experiments go wholly to confirm the observation of Bárány which was given above. And we must now consider the precisely contradictory testimony of Mach. Since there is not the slightest ambiguity in the form of his statements, there remain three conceivable ways of reconciling them with the other observations above reported. It is possible that with Mach and his subjects the nystagmus was not really inhibited, for not all who try to inhibit it succeed. This is rendered

plausible by the fact that Mach nowhere speaks of inhibiting the nystagmus by fixating toward the *side* contrary to motion, and in the absence of such a statement one must suppose that the inhibition of nystagmus was attempted by trying to hold the eyes voluntarily in the primary position, *i. e.*, straight forward (cf. the above quotation from Mach). Now I am personally quite unable to inhibit the nystagmus, either during or after rotation, in this way, nor have I seen another person who was able to do this; and it is clear from Bárány's observations ('06, S. 215–17) that such an attempted fixation straight ahead might actually augment rather than decrease the nystagmus. Nor can I attach much importance to the after-image test above quoted, with the black cross on a white ground, since there is no vision during the quick phase of the nystagmus, as we shall see later, and since the slow phase is too slow to leave a perceptible after-image streak unless the stimulus (here the 'white ground') is very intense.

Yet aside from this Mach says that he examined the eyes of another observer during the voluntary inhibition. And while this is very difficult, since Mach himself must also rotate, while also nystagmic movements sufficient to produce dizziness can be so minute as to need a reading telescope for their discovery (Bárány, '06, S. 214), I cannot think it probable that Mach would have convinced himself, as he says, that this observer had inhibited the nystagmus if such had not really been the case.

A second possibility would be that Mach and his subjects mistook centrifugal sensations (group 2) for the true movement sensations of group 3. (Sensations of group 1 give no feeling of *bodily* motion so long as the nystagmus is inhibited.) This would be very probable with observers of little experience, but it can hardly have happened with Mach; and furthermore he says that visual dizziness also continues after the inhibition of nystagmus, and centrifugal sensations could scarcely have been mistaken for visual dizziness.

The third alternative remains, that we have here a true case of individual difference. And one must be the more willing to admit this here since it is not more remarkable than other mani-

fest discrepancies among the observations of careful experimenters in this same field. Thus Bárány, for instance, gets post-rotary dizziness of his body so long as his eyes are closed, but this is *supplanted* by visual dizziness when he opens his eyes: whereas Mach, and most other observers, feel with the eyes open both kinds of post-rotary dizziness *at once* (Bárány, '06, S. 223). Or again, in post-rotary visual dizziness Bárány sees the visual field oscillate from side to side in both directions ('06, S. 221); whereas Mach, Breuer, Delage and most other observers see it whirl contrariwise to the preceding actual rotation (Nagel, '05; Peters, '05); and Helmholtz ('67, S. 603; '96, S. 747) saw it whirl sometimes with and sometimes contrary to, the direction of the preceding rotation. Still more extraordinary are the different observations as to the localization of a visual after-image with the eyes closed, during voluntary and involuntary eye-movements. Indeed there are few branches of psychology where entirely credible observers more widely disagree regarding simple matters of fact. And I should designate this branch as the one comprising the following four things and their interrelations — motion, muscular contraction, the voluntary innervation to contraction, and the perception of movement.

Granted, then, the fact of unusual individual or typical differences, it remains to study the several types in and for themselves, in the anticipation that in the end some explanation will be found which will reconcile all discrepancies. I have not so far seen a subject who, like Mach, experiences bodily and visual dizziness after he has inhibited his ocular nystagmus, but I shall look for such persons, and meanwhile return to the discussion of such cases as Bárány and I have met. For some things are inevitably true of these subjects, whatsoever else may be true of the members of other types. Now we have seen that for the subjects who are at present in question, voluntary inhibition of the nystagmus inhibits the sensation of bodily rotation. Bárány ('06, S. 275–6) has sought to interpret this fact: "We have further seen that inhibition of the nystagmus eliminates the sensation of rotation. Since the voluntary direction of the regard can scarcely effect an inhibition of impulses coming over

the vestibular nerve, we seem bound to conclude that the nystagmus as such, the involuntary and unconscious movement of the eyes, is of influence on the production of the rotary sensation; perhaps, indeed, that it is the nystagmus center in which the abovementioned integration [Verarbeitung] of the vestibular impulses and the excitations occasioned by changes of the head position, takes place. I perhaps went too far in an earlier paper, where I said that inhibition of the rotary sensation through inhibition of the nystagmus, proves that the nystagmus and not the vestibular impulses cause the rotary sensation. *It is sufficient to assume that for the production of rotary sensation such impulses are necessary as, owing to the nystagmus, are delivered to the center involved, and that these form so large a component in the integration of the subcortical impulses [?] that the inhibition of them suffices to prevent the sensation of rotation from being produced. Nothing but a case of total, bilateral, oculomotor paralysis of central origin could really decide the point: in such a case there ought to be no sensation of rotation.*" I quote this passage in full (with italics as in the original) because it bears so explicity on our theme. I understand Bárány's conception to be that afferent vestibular impulses and afferent impulses from eye-movements are combined in a subcortical center, from which they emerge in consciousness *as* the sensation of rotation: and that the latter components (which would ordinarily be called 'sensations of eye-movement') at least are indispensable to the production of rotary sensations. Presumably he would hold the vestibular impulses to be indispensable as well.

But there are alternative possibilities. Is it true, as he declares, that "the voluntary direction of the regard can scarcely effect an inhibition of impulses coming over the vestibular nerve"? The voluntary direction of the regard certainly inhibits whatever impulses those are which produce the *rapid* phase of the nystagmic movement, and I see nothing to warrant a statement on one side or the other as to the relation between the vestibular and the voluntary impulses. It *might* be that the vestibular sensations are the sensations of rotation, but that these are inhibited when the rapid eye jerks are inhibited. And yet on the one hand professed ignorance is better than so far-

fetched and mysterious an assumption as this latter; while on the other hand, regarding the former assumption, we must remember the many cases in which one's body is felt to move, with sensations distinctly like those of group 3, in which neither the semicircular canals nor the sacs can be supposed to be stimulated. A person who stands on a bridge and watches the water flow beneath, from time to time feels himself moving contrary to the flow of the water (Mach, '00, S. 104). Helmholtz ('96, S. 763–4) mentions that when the dome of an astronomical observatory is turned about, a person standing beneath it is apt to feel the floor and himself rotating contrariwise. And there are many other such illusions of bodily translation or rotation, not distinguishable from the sensations of group 3, in all of which the stimulus is purely visual and *there are no* afferent vestibular impulses. These considerations, I believe, quite shut out the vestibular impulses from being essential to the sensation of motion of one's own body.

We have next to examine Bárány's second and indispensable component — the afferent impulses occasioned by the nystagmic movements. Before we can suppose such impulses to be essential, or even in any wise contributory to the perception of motion, we must answer satisfactorily the arguments so cogently, and one might almost say savagely, stated by Hering ('61, S. 30–32): They are, he says, "proof enough that only the displacement of retinal images, and not sensations of tensions in the muscles, acquaint me with changes in the position of my eyes." In short, Hering allows no share at all to eye-muscle sensations in the perception of eye-movement; and he is disinclined to allow even their existence. Is it, then, conceivable that they afford sensations of movements of the whole body? We must also bear in mind that the famous discussion between Plateau, Oppel, Helmholtz, Dvořák and others did not confirm the belief in the existence of eye-muscle sensations. We have also had recently, from Dodge ('04 and '07), Judd ('05) and other investigators in the Yale Laboratory, fresh evidence that sensations of eye-movement play little or no part in the perception of space. Personally I am unable at present to dispense with 'eye-movement sensations' as a part of my psychological

furniture, and yet in the present case I must admit that several facts seem to exclude them from assuming any importance. It is, apparently, the efferent motor impulses to the eyes rather than afferent impulses yielded by eye-movements that have taken place, that most closely parallel, or as some persons might say, *are*, the sensations of bodily movement.

These facts are, firstly, that of the two phases exhibited by the nystagmus, the rapid and the slow, one phase but not the other seems to cause sensations of bodily movement. This is the rapid phase. Under positive acceleration the rapid eye-jerk is *with* the rotation, and in this same direction the body is *felt* to turn; and as soon as the acceleration becomes negative the rapid jerk takes place contrary to rotation, and the body is felt to reverse its motion although it is actually rotating in the same direction. At high but *uniform* speed, of course, there is no nystagmus and likewise no sensation of one's body being in motion. As Bárány has said ('06, S. 225), "*the apparent rotation of one's own body is always in the same direction as the rapid nystagmic movement.*" Now on the sensory side there is nothing, so far as we know at present, to distinguish these movements so sharply from each other; for of course the circumstance that one is fast and the other slow would not account for one of them being 'sensed' and the other not. But on the efferent side there is a prime distinction — *the motor impulses to the rapid movement can be voluntarily inhibited*, while those to the slow cannot be checked (Holt, '06, p. 72). The voluntary attempt to inhibit them, which has to be made by trying to fix the regard in a direction wholly or partly opposed to the direction of the slow movement, results only in *increasing* the nystagmus (Bárány, '06, S. 215–17). (This is why Mach is trying to fixate a point straight in front, is unusually fortunate if he succeeded in inhibiting the nystagmus.) Now certainly the rapid movement cannot be called a voluntary movement, since the whole nystagmus arises involuntarily; but since the rapid phase is amenable to voluntary inhibition, it may properly be called semi-voluntary. Now this circumstance that the rapid phase which alone counts toward the sensation of bodily movement is more nearly related to voluntary effort than the other

phase, is directly in line with those facts already referred to, which Hering so emphasized ('61, S. 30). "A position of the eyes which I have not voluntarily induced, which therefore I did not already know pretty exactly before it took place, and even more a movement which takes place without my special intention — I am totally unable to estimate. . . . [S. 31] If I have voluntarily brought about a position of the eyes, then of course I know beforehand the direction and approximate extent of movement [involved], since otherwise I should not have been able to will just this movement; and both the direction and approximately the amount of force which is necessary for a given movement, are decided [bestimmt] by the will." This is coming, perhaps, very close to 'innervation feelings.' The empirical data to which Hering refers, may be summarized as the general lack of relation between the position of the eyes and the subjective localization of optical impressions. The eye-movement sensations, supposing them to exist, in many cases so inadequately register the eye-movements that a false localization is assigned to the objective sources of visual data. An example will sufficiently illustrate this general phenomenon.

In a previous paper ('06, p. 72) I observed, "it is well known that after-images move with every involuntary eye-movement." Here I relied chiefly on the introspection during dizziness (with eyelids closed) of myself and four other subjects (*ibid.*, p. 70–1), on some observations of my own on the vision of after-images during 'pursuit movements' (Dodge, '03), and I *think* on some printed statements which, however, I can no longer identify. My own observations were, and on retrial still are, unequivocal. But I have since discovered that other observers equally 'well know' that for them after-images do *not* move with involuntary eye-movements: on this point both Hering ('61, S. 30–31) and Bárány ('06, S. 221–2) are perfectly explicit, and several other authors imply the same view. On the other hand Hering's account distinctly implies that after-images *do* move with *voluntary* eye-movements even when the eyelids are closed. Bárány ('06, S. 222) gives this as Hering's view and confirms it himself (S. 221), Exner ('90, S. 50) and A. Nagel ('71, S. 256) affirm the very same. Whereas

Purkinje (Aubert, '88, S. 118) recounts and apparently confirms an experiment of Charles Bell's in which "a blindingly bright image left on the eye after gazing on a shining object, always appears at rest during eye-movements executed in total darkness, and starts into motion only when the eyes are open and the [after-]image can be compared with external objects which are at rest." Now the observations in question are singularly easy to make, even for a novice, and I believe that such extraordinary discrepancies again rest on true differences between observers. (A colleague of long experience in the study of vision tells me that he gets both of the last-mentioned phenomena, but more commonly the latter of these.) Now these discrepancies, if we accept them, prove that there is no direct relation between eye-movements and the localization of after-images, and such a thing is scarcely possible if visual data in general are localized by means of eye-movement sensations. But the main differentia, of which we know, between the above cases lies in the manner of *innervation* — whether this is voluntary, semi-voluntary, or involuntary: the first alone affecting the localization of after-images for Hering and Bárány. And with this view Mach ('00, S. 93–105) also concurs. It is true that this does not remove all disagreement, for I find on myself and four subjects, that localization of after-images shifts with both phases (semi-voluntary and voluntary) of labyrinthine nystagmus, while Hering and Bárány deny this; and Purkinje and Bell find that even voluntary innervation does not affect the localization of after-images. Yet it is, to my mind, simpler to suppose that these divergencies rest on peculiarities of innervation than of eye-movement sensation. And in this Bárány, even more emphatically Hering, and also Mach would agree.

One can of course assume the inhibition, under given circumstances, of the supposed eye-movement sensations and hence their failure to govern localization; but there is nothing to inhibit them in our examples above save the several modes of innervation: so that again the explanation of the phenomenon would lie in peculiarities of innervation. Furthermore the observation of Mach, already described, that the feeling of both bodily and visual dizziness (rotary localization) *can* persist when

there are no nystagmic movements could not be explained in terms of sensation of these non-occurring movements. And if recourse is then had to reproduced sensations of eye-movement, it again appears that only the labyrinthine or voluntary innervations could be effecting such reproduction. The fact that Lotze, Münsterberg and others have declared 'innervation feelings' *to be* reproduced sensations of movement does not affect the present case for the issue here is between afferent and efferent process,— whether incoming or outgoing impulses are more nearly parallel to feelings of motion. To resort, then, to *re*produced kinæsthetic sensations is to yield the point at once and to grant that it is outgoing impulses and not impulses coming in from the eye-movements that govern the consciousness of movement. And while in Mach's experiment his feeling of bodily and visual dizziness cannot have come from sensations of eye-movement, since he says that the eyes were not moving, it may well have come from innervations to eye-movement, innervations which became inhibited at some level lower than their point of origin. It is worth noting that this view, and so far as I know no other view, accounts for the familiar pathological cases in which the innervation to contract a muscle which is paralyzed, produces the feeling of *the intended* movement although the muscle is not actually contracted by the innervation.

If now visual localization is not explicable in terms of eye-movement sensations, the localization of one's body is of course even less so. And thus this latter depends neither on afferent impulses from the eye-balls nor, as we saw before, on afferent impulses from the labyrinth; and yet the voluntary inhibition of eye nystagmus *inhibits* the feeling of movement (changing localization) of one's body. Only one conclusion remains — the voluntary innervation to inhibit the nystagmus, which is really directed as we have seen to inhibiting the *rapid phase* of the nystagmus, suppresses the feeling of bodily motion by inhibiting the (semi-voluntary) *innervation* of that rapid phase. And it will be recalled that the feeling of motion of one's own body is always in the same direction as the rapid phase. Hence it is the semi-voluntary innervation to the rapid nystagmic phase

which is the process most closely parallel to the feeling of bodily rotation. Our first problem was: "*What organs yield the sensation of rotation?*" *And the answer would be that this is not a sensation in the ordinary meaning of that word, but that the process most nearly parallel to the feeling of rotation is one kind of innervation process.* And I believe that this proposition applies to more than rotation, applies at least to all feelings of motion of one's body that are supposed to be given by the semicircular canals.

This result is nearly in line with a view long supported by Mach ('00, S. 95), who says: "The will to execute movements of regard [Blickbewegungen] or the innervation (?) to these, is the very sensation of space itself." The question-mark after the word 'innervation' is presumably out of deference to those who oppose 'innervation feelings.' I should not care to say innervations *are* sensations of space nor, for reasons too general to be discussed here, that innervations *are* the feelings of bodily movement. The phrasing as italicized above is, I think, a somewhat securer statement. But it is clear that our argument, based in several places on observations at variance with those of Mach, comes out to a position not so far removed from his.

It is true that I have not explained all the conflicting observations given above, nor have I, by-the-way, begun to exhaust the anomalies that stand on record in this field. But I believe that the cases which we have considered, if they are facts and not errors of observation on the part of one person or another, necessitate the conclusion to which we have arrived. And whatsoever conclusion other facts lead to, it will not be contradictory to this of ours. We have found empirical evidence of three grades of innervation — voluntary, semi-voluntary, and involuntary — and that these exert a different influence over the inhibition of the feeling of movement. It is therefore probable that different grades of innervation are of different value in producing the feeling of movement. The three grades of innervation doubtless emerge at higher and lower neural levels; but the neural levels are many and hence the grades of innervation may be many. On this, I think, we may base a reasonable

hope of explaining the so-far complicated and seemingly conflicting mass of observations.

The further problems raised in the first part of this paper must be discussed in a sequel, and it here remains to say only this: The conclusion that *innervations* of one kind or another are the process most nearly parallel to feelings of bodily motion, does not, of course, imply that such efferent impulses are created from nothing, as say on the 'psychic plane.' The nervous energy that constitutes these innervations is released by impulses coming more or less immediately from the periphery. For the physiological unit of the nervous system is the reflex arc. And the issue raised by our empirical data between afferent (from the eye-muscles or orbit) and efferent (to the same) is, I apprehend, more precisely stated as follows. Is the nervous process which runs parallel to the consciousness of the rotation or translation of one's body, one in which the afferent or sensory impulses come wholly or mainly from eye-muscles (or orbit), and diverge in the central nervous system, passing out as diffuse innervations to various and so far unidentified members; or is the process one in which the afferent impulses come from various and so far unidentified members, and converge in the central nervous system, passing out as a unified and definite innervation to eye-movement? The latter alternative is the conclusion that we have reached.

Bibliography.

Abels, H.

'06. Ueber Nachempfindungen im Gebiete des kinästhetischen und statischen Sinnes. *Zeitschr. f. Psych.*, 43, 1906, S. 268–289; S. 374–422.

'07. Ist der 'Nachschwindel' im Endorgan oder nervös bedingt? *Ibid.*, 45, 1907, S. 85–91.

Aubert, H.

'88. Physiologische Studien über die Orientierung. Tübingen, 1888.

Bárány, R.

'06. Untersuchungen über den vom Vestibularapparat des Ohres reflektorisch ausgelösten rhythmischen Nystagmus und seine Begleiterscheinungen, *Monatsschr. f. Ohrenheilk.*, 40, 1906, S. 193–297.

Breuer, J.

'07. Bemerkungen zu Dr. Hans Abels Abhandlung, etc. *Zeitschr. f. Psych.*, 45, 1907, S. 78–84.

'90. Ueber die Function der Otolithen-Apparate. *Pflüger's Archiv f. d. ges. Physiol.*, 48, 1890, S. 195–306.

BROWN, A. CRUM.

'95. The Relation between the Movements of the Eyes and the Movements of the Head. Robert Boyle Lecture, London, 1895.

DELAGES, Y.

'86. Études expérimentales sur les illusions statiques et dynamiques de direction, etc. *Archives de Zool. exp. et générale*, 2 ième Série, 4, 1886, pp. 535–624.

DODGE, R.

'03. Five Types of Eye Movement, etc. *Amer. Jour. of Physiol.*, 8, 1903, pp. 307–29.

'04. Participation of the Eye Movements in the Visual Perception of Motion. *Psych. Rev.*, 2, 1904, p. 1.

'07. An Experimental Study of Visual Fixation. *Studies fr. the Psych. Lab. of Wesleyan Univ.*, 1, 1907, pp. 1–95.

EWALD, J. R.

'92. Physiologische Untersuchungen über das Endorgan des Nervus Octavus. Wiesbaden, 1892.

EXNER, S.

'90. Das Verschwinden der Nachbilder bei Augenbewegungen. *Zeitschr. f. Psych.*, 1, 1890, S. 47–51.

HELMHOLTZ, H.

'67. Handbuch der Physiologischen Optik. Leipzig, 1867.

'96. Handbuch der Physiologischen Optik. Hamburg und Leipzig, 1896.

HERING, E.

'61. Beiträge zur Physiologie. Leipzig, 1861.

HOLT, E. B.

'03. Eye-Movement and Central Anæsthesia. *Harvard Psych. Studies*, 1, 1903, pp. 3–45.

'06. Eye-Movements during Dizziness. *Ibid.*, 2, 1906, pp. 57–66.

'06. Vision during Dizziness. *Ibid.*, pp. 67–73.

JUDD, C. H.

'05. Movement and Consciousness. *Yale Psych. Studies*, New Series, 1, 1905, pp. 199–226.

MACH, E.

'73. Physikalische Versuche über den Gleichgewichtssinn des Menschen. *Sitzungsber d. kais. Akad. d. Wiss. Wien, math.-naturw. Cl.*, 68, 3te Abth., 1873, S. 124–140

'74. Versuche über den Gleichgewichtssinn. *Ibid.*, 69, 2te Abth., 1874, S. 121–135

'00. Analyse der Empfindungen. 2te Aufl., Jena, 1900.

MÜNSTERBERG, H., and PIERCE, A. H.

'94. The Localization of Sound. *Psych. Rev.*, 1, 1894, pp. 461–476.

NAGEL, W.

'05. Die Lage-, Bewegungs- und Widerstandsempfindungen. *Handb. d. Physiol. d. Menschen. Braunschweig*, 2, 1905, S. 734–806.

PETERS, W.

'05. Die Bewegungs- und Lageempfindungen. *Archiv f. d. ges. Psych.*, 5, 1905, S. 42–76.

PIERCE, A. H.

See Münsterberg, H.

PURKINJE.

'25. Viertes Bulletin d. naturw. Sektion d. schles. Gesellsch. f. väterl. Kultur, 1825.

'26. Zehntes ditto, 1826.

'26. Zweites Bull. d. naturw. u. botan. Sektion d. schles. Gesellsch. f. väterl. Kultur, 1826. (The above three Bulletins are reprinted by Aubert, '88.)

SCHÄFER, K.

'87. Ueber die Wahrnehmung eigener passiver Bewegungen durch den Muskelsinn. *Pflüger's Archiv f. d. ges. Physiol.*, 41, 1887, S. 566.

SHERRINGTON, C. S.

'06. The Integrative Action of the Nervous System. New York, 1906.

WUNDT, W.

'02. Grundzüge der physiologischen Psychologie. 5te Aufl., Leipzig 1902-3.

No. 4 24

[Reprinted from the PSYCHOLOGICAL REVIEW, Vol. XVIII., No. 1, Jan., 1911.]

EXPERIMENTS ON THE INHIBITION OF SENSATIONS[1]

BY EDMUND JACOBSON

§1. *Introduction.*—The inquiry as to how simultaneous sensations affect each other has an obviously fundamental importance. With apposite technique Heymans has shown that threshold sensations of color, taste, sound, pressure and light may be totally suppressed by other strong sensations respectively of the same quality. He has found also that the faint sensations caused by the ticking of a watch may be inhibited by applying a faradizing current to the fingers. On detailed observations he has based several laws, the first being that the inhibitory power of a sensation increases in proportion to its intensity, that increase being measured in terms of the stimuli to the sensations which it is able to suppress.[2]

Heymans assumes without comment that when faint sensations are inhibited, this is equivalent to a diminution of their intensity. In this he is simply accepting the word of his subject, who with his attention voluntarily directed to the sense-organ finds them weakened or absent. Were one to suppose that the faint sensation is present in the subject's consciousness, but that he is unable to get at it, one would be

[1]From the Harvard Psychological Laboratory; being a revision of an experimental chapter of a thesis on 'Inhibition' submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy at Harvard University, June, 1910.

[2]For these laws he claims a far-reaching character, applying them for the explanation of the basis of sensational thresholds. In addition he calls attention to other experiments—certain ones made by himself and certain ones made by others—the results of which he interprets as showing that sensations of difference ('*Unterschiedsempfindungen*') both weak and strong, can be partially or totally suppressed by ordinary sensations of vision or the like or by other sensations of difference. This being so, he sees in it the explanation of Weber's law. Our present concern lies only with the first law. 'Untersuchungen über psychische Hemmung,' *Zeitschr. f. Psychol.*, 21, 1899, 321 f.; *ibid.*, 26, 1901, 305 f.; *ibid.*, 34, 1904, 15 f.

implicitly asserting the existence of an unconscious sensation,—which is an evident contradiction. The intensity of a sensation is precisely that in it which tends to increase if, for example, the corresponding stimulus is increased; and what Heymans finds is that he may increase the stimulus and yet the faint sensation will fail to appear in consciousness if also he sufficiently increases the stimulus to the stronger inhibiting sensation. Hence it is with justice that he assumes that the strong sensation decreases the intensity of the faint one.

Granting, however, that liminal sensations may be obliterated by other but powerful sensations either of like or of unlike quality, for instance, that one produced by a few hundred milligrams of pressure might vanish if a three hundred gram weight were applied to the skin near by—further research seems necessary in order with surety to warrant such assertions as that the three hundred gram weight would occasion an appreciable loss in a sensation far above the limen in strength, for instance, in one produced by a 20-gram weight. For it is easily conceivable that the latter might be powerful enough effectively to resist even partial inhibition; and even if it did suffer a diminution equivalent to a few hundred milligrams, we might anticipate that this loss would not be noticeable, according to the well-known data that center about Weber's law. And the whole situation is further complicated by the fact that other observers than Heymans have found that distractives may sometimes fail of their purpose and work augmentor effects instead of inhibitory. Hence it seems interesting to inquire how moderately strong sensations would be affected, if at all, by other simultaneous sensations.

First of all the problem arises as to some means for measuring the sensations in the subject's mind. At the same time, the well-known historical controversies about whether increased attention brings increased intensity to sensation make it clear that all attempts to come to definite solutions through direct appeals to the introspection of the subject would be likely to prove futile. Accordingly, it is necessary

to find some other means for measuring changes in intensity, some relatively objective scale or standard. Such an objective scale or standard is furnished in regard to any given sensation by comparing it with others subsequently or previously given and determining how many times out of ten it is judged stronger than they. If, for instance, a pressure sensation produced by 20 grams is to be used, then compare it with sensations produced by weights of 12, 15, 17, 18, 21, 23, 25, 27 and the like. In this manner one can find a rough scale in which the 20-gram weight will be judged heavier, say 10, 9, 6, 5, 4, 2, 1, 0 times out of ten than the other weights. One may now choose 2 weights C' and C such that the one is somewhat heavier than 20 gr. and the other somewhat lighter, and hence that the 20-gr. weight on the average is judged heavier than C' from 1 to 3 times out of ten and heavier than C from 6 to 9 times out of ten.[1] Then a sound may be produced simultaneously with the sensation occasioned by the application of C' or C, and the number of times out of ten that the 20-gr. weight (given always without the sound) is now judged heavier than C' and C may be determined. If the 20-gr. weight is judged heavier than C' more times when the sound is applied with the latter weight than when not, one would have some reason to believe that the sound diminished the sensation of C'. Yet this same result might be consistent with the very different assumption that the sound simply confused the judgment and rendered discrimination less exact. If 20 gr. is judged heavier than C' only two times out of ten, then any disturbance of the exactitude of judgment might conceivably lead to an average increase to three or four times out of ten. But suppose that in addition to showing that the 20-gr. weight is judged heavier than C' with the sound more often than without, the statistics showed also that it was judged heavier than C more often when the sound occurred simultaneously with C than when not: then the result would be unequivocal. For the figure for C is already about eight out of ten, and there would be no reason for sup-

[1]The standard weight which we arbitrarily name here as 20 gr. may of course have other denominations. In our later discussions we shall call it S, leaving C to tand for the lighter weight that is compared with S, and C' for the heavier weight.

posing that disturbance of discrimination would increase this number on the average still nearer to ten. Far more would it be to be expected that poorer discrimination would tend to make the subject unable to call 20 gr. heavier than C so many as eight times out of ten; or else that it would increase the mean variation about eight. But if not alone the figure for C' but also that for C is increased, then there could remain no reasonable doubt that since the sound makes the heavier weight reported lighter and the lighter weight even lighter still than before, we might infer that the sound in some way diminishes the weight sensation. This method of inquiry lies at the basis of Series 1.

Series 1.

Inhibition of Pressure Sensations by Sound Sensations.

§2. *Apparatus.*—Sound sensations of moderately strong intensity were produced by means of an electrical buzzer always placed 30 inches from the left ear of the subject, whose head was secured by means of a head-rest. The sound consisted essentially of loud clicks in so quick succession as almost to be fused. The sound varied somewhat from day to day, particularly because of variations in the current, but it was sufficiently satisfactory for the present purpose.

Pressure sensations were produced by means of the instrument used by Kobylecki and originally invented in a simpler form by Stratton. The former has given a full description of it in *Wundt's Psychologische Studien*, Vol. 1, and it will suffice here to give a simple explanatory sketch of the machine as it was adapted for the purposes of Series 1 (cf. Fig. 1). The subject sat facing the machine with elbow supported, his right hand placed palm downward on the rest indicated under T. As will be seen, only two levers, AB and DC, are used, and these are evenly balanced and move freely. At the point A there projects downward a rounded stick of light wood. The lower end of T did not come in contact with the hand, but rested on a cork disc about 15 mm. in diameter, the position of which was usually somewhere between the knuckles of the middle and fore-finger. Other locations,

also, were used at times. It will be apparent from the sketch that a weight placed on the pan *R* will cause *T* to move downwards and thus communicate pressure to the cork disc on the hand. It is advisable, however, to have the lower end of *T* always in contact with the cork disc, else the application of the weight to the pan *R* will cause *T* to fall suddenly and thus give a sensation of impact instead of mere pressure. The screw *K* can be used for this purpose but it works very

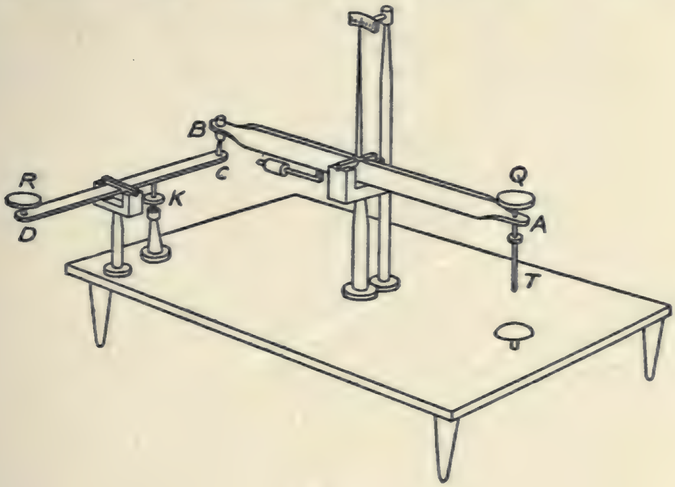


FIG. 1.

badly in that the subject's hand often shifts slightly or changes in volume, when the lower end of *T* may easily come to lose contact with the cork, or on the other hand, may come to press tightly on it. Hence *K* was not used at all, but instead a 3-gr. weight was allowed always to remain on the pan *R*, and this kept *T* in continual light contact with the cork in a very satisfactory manner. The weights were applied to the pan *R* by putting them on a piece of stiff but soft blotting paper and lowering them gently to the pan, a thing which required much practice on the part of the operator.

§3. *Procedure.*—The operator gave a signal by saying 'One!' in a low voice and then placed the weight on the pan *R*, removing it after four seconds. After an interval also of four seconds, he gave the signal 'Nine!' and placed the second

weight on the pan, removing it again after four seconds. The subject pronounced judgment with respect to the second weight saying that it was heavier, lighter or the same as the first.

The session with each subject occurred once per week, the actual work lasting usually about forty or forty-five minutes. The first part of the period usually was devoted to practice, but the number of judgments recorded in a successful session averaged between forty and eighty. During the session rest-periods of a few minutes each were given,—sometimes one or three, but more often two. Nevertheless there were times when the subject complained of cramp of the arm, which came from keeping it in a constant position. While fatigue of the sense-organ had to be taken into account and as much as possible avoided, yet it seemed to present no grave difficulty. The position of the cork disc on the hand was changed now and then at the instance either of the operator or of the subject, the latter being requested to move the disc when it seemed necessary.[1] When the attention was good, the judgments proved on the whole as reliable toward the end of the hour as at the beginning. We may add the detail that there was daylight illumination, but that the subject sat with closed eyes. Unfortunately the room was not sound-proof, and hence it was frequently necessary to wait until some outside noise should cease, and even to repeat a set of weights which had been judged. A repetition was also made whenever in the judgment of the subject or operator the weights were put on the pan in improper manner. Repetitions were specially recorded in the protocol except in the first part of Series I. A special sign was used to mark whenever the subject found that owing to poor attention or the like he was in doubt and unable to judge. Every effect was made to have him properly distinguish between cases in which he ought to judge 'the same' and those in which he ought to report himself really unable to judge. In all likelihood, however, many cases were pronounced 'doubtful' which should have been called 'the same' and *vice versa*, for the distinction is often very difficult

[1]Rests and changes of the position of the cork were recorded in the protocol.

indeed. It scarcely need be added that the operator continually had in mind the danger of unconsciously communicating to the subject anything that might influence his judgments. Hence care was taken in uttering the signals and in other similar respects. The personal equation of the operator in this series certainly introduces a large error, but in a subsequent series where sounds mechanically produced were judged instead of weights, the general character of the results obtained were the same as those of this series, and hence that error may to some extent be disregarded. So far as possible, the ultimate aims of the experiment were kept from the knowledge of the subject.

It will be recalled that the method of procedure is to select a convenient weight S which produces a pressure sensation of moderate strength and then to find two other weights C and C', one lighter and the other heavier, as described on page 26. Because of the well-known tendency to overestimate the strength of the second of two sensations, it is necessary to distinguish between the orders $S–C$ and $C–S$. For if S is judged heavier than C a certain number of times out of ten when given in the former order, one may expect it to be judged heavier a different number of times out of ten in the latter order.[1] We have, then, the couplets $S–C$, $C–S$, $S–C'$ and $C'–S$, which we shall designate respectively by the letters A, B, E and F. Our purpose is to find the number of times out of ten that S is judged heavier than C or C', in each of these orders, and then to produce the sound simultaneously with C and C' (but not with S) and to find how many times S is then judged heavier. Accordingly, if we let the symbol $/n$ stand for the occurrence of the sound, we have also the couplets $S–C/n$, $C/n–S$, $S–C'/n$, and $C'/n–S$, which we shall designate respectively by the letters C, D, G and H. Because of personal peculiarities of sensibilitity and discrimination power, different sets of weights were used for each subject, and this is indicated in Table I. For example, it will be seen that for subject 1 $S = 14$, $C = 10$, and $C' = 18$, while for subject 2 $S = 20$, $C = 15$, and $C' = 25$.

[1]Usually a larger number of times in the order $C–S$, according to my own experience.

TABLE I.[1]

	Subject 1.		Subject 2.		Subject 3.		Subject 4.		Subject 5.	
	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).
A	14	10	20	15	22	18	24	18	20	10
B	10	14	15	20	18	22	18	24	10	20
C	14	10/*n*	20	15/*n*	22	18/*n*	24	18/*n*	20	10/*n*
D	10/*n*	14	15/*n*	20	18/*n*	22	18/*n*	24	10/*n*	20
E	14	18	20	25	22	26	24	30	20	30
F	18	14	25	20	26	22	30	24	30	20
G	14	18/*n*	20	25/*n*	22	26/*n*	24	30/*n*	20	30/*n*
H	18/*n*	14	25/*n*	20	26/*n*	22	30/*n*	24	30/*n*	20

Since the subject is instructed to pass judgment in terms of the second weight given it will be apparent that if we count the number of judgments 'lighter' in the cases of A, C, E and G, but the number of judgments 'heavier' in the cases of B, D, F and H, we shall in each case have a figure representing the number of times that *S* is judged heavier than its mate. It is obvious also that A is to be compared with C, B with D, E with G, and F with H; or similarly, (A+B) with (C+D), and (E+F) with (G+H); or again, (A+B+E+F) with (C+D+G+H). Following a practice that is general in experiments with weight sensations, we reckon the judgment 'same' as equivalent to one half 'heavier' or one half 'lighter.' In other words 'same' means midway between 'heavier' and 'lighter.' Series 1 consisted of four parts in each of which ten judgments were passed on each of the couplets A, B, C, D, E, F, G, H given in haphazard order. Hence altogether there were 4×10 judgments passed on each of eight couplets, making a total of 320 judgments for each subject.

§4. *Results.*—The results of Series 1 are presented in Table II. As the explanatory note under the table indicates, (which see) each figure in roman type is to be compared with the corresponding one in italic. Even a careless glance will show that the italic figures are in a good majority of the cases larger than those in roman. Out of the eighty cases in this table where one may compare the figure for a single letter in roman (A or B or E or F) with the corresponding one in italic, an increase occurs

[1]The sign (/*n*) signifies the occurrence of the sound.

53 times, an equality 14 times, and a decrease only 13 times. The increase is made especially clear in the sums in the table. It can also be shown in the averages;—for instance, the total averages for all subjects reckoned together are as follows: A = 7.5, B = 8.2, C = 8., D = 8.7, E = 1.7, F = 3.5, G = 2.8, H = 4.6.

TABLE II.

		Subject 1.		Subject 2.		Subject 3.		Subject 4.		Subject 5.	
Series 1–A.											
A	*C*	9	*10*	4½	*8*	8½	*8*	7	*7*	9	*9*
B	*D*	9	*9*	9	*9*	8	*8*	7	*7½*	9	*10*
A+B	*C+D*	18	*19*	13½	*17*	16½	*16*	14	*14½*	18	*19*
E	*G*	3	*5*	0	*1½*	2	*5*	6	*3½*	0	*2½*
F	*H*	3	*4*	5½	*4½*	6	*8*	1½	*3*	3	*4½*
E+F	*G+H*	6	*9*	5½	*6*	8	*13*	7½	*6½*	3	*7*
A+B+E+F	*C+D+G+H*	24	*28*	19	*23*	24½	*29*	21½	*21*	21	*26*
Series 1–B.											
A	*C*	8	*9*	3½	*5*	5	*7*	8	*8*	9½	*8*
B	*D*	8	*8½*	10	*10*	9	*6*	7½	*10*	10	*9½*
A+B	*C+D*	16	*17½*	13½	*15*	14	*13*	15½	*18*	19½	*17½*
E	*G*	1	*2*	1	*6½*	4	*2*	½	*2*	2	*1*
F	*H*	4½	*5*	5	*7½*	4	*9*	4	*4*	2	*2*
E+F	*G+H*	5½	*7*	6	*14*	8	*11*	4½	*6*	4	*3*
A+B+E+F	*C+D+G+H*	21½	*24½*	19½	*29*	22	*24*	20	*24*	23½	*20½*
Series 1–C.											
A	*C*	9½	*7½*	8	*9½*	7	*8*	6½	*8*	7½	*9½*
B	*D*	7½	*9*	9	*7*	8	*9*	10	*10*	10	*10*
A+B	*C+D*	17	*16½*	17	*16½*	15	*17*	16½	*18*	17½	*19½*
E	*G*	1	*5*	4	*4*	2½	*4*	0	*½*	½	*1½*
F	*H*	2½	*3*	4½	*5½*	3	*4*	4	*2*	2½	*5½*
E+F	*G+H*	3½	*8*	8½	*9½*	5½	*8*	4	*2½*	3	*7*
A+B+E+F	*C+D+G+H*	20½	*24½*	25½	*26*	20½	*25*	20½	*20½*	20½	*26½*
Series 1–D.											
A	*C*	8½	*9*	8	*8½*	7	*5*	7	*8½*	8	*8*
B	*D*	5	*8*	8	*5½*	4	*9*	8	*9*	8½	*10*
A+B	*C+D*	13½	*17*	16	*14*	11	*14*	15	*17½*	16½	*18*
E	*G*	4	*2½*	0	*3½*	1	*2*	½	*1½*	1	*1*
F	*H*	2½	*4*	2	*2*	4	*5*	3	*5*	3½	*4½*
E+F	*G+H*	6½	*6½*	2	*5½*	5	*7*	3½	*6½*	4½	*5½*
A+B+E+F	*C+D+G+H*	20	*23½*	18	*19½*	16	*21*	18½	*24*	21	*23½*
Total sum	*Total sum*	86	*100½*	82	*97½*	83	*99*	80½	*89½*	86	*96½*

Figures opposite single letters in roman type (A or B or E or F) in each case represent the number of times a given weight (*S*) was judged heavier than another (*C* or *C′*); while in the space to the right of each such figure in roman is a corresponding figure in italic which represents the number of times that same weight (*S*) was judged heavier than the same other one (*C* or *C′*) when the sound was made simultaneously with the latter.

In other words, the number of times that a given weight, S, was judged heavier than another was on the whole increased when a sound was made simultaneously with the application of that other. That the influence of the sound was not essentially to confuse the judgment was learned firstly from the reports of the subjects who soon became accustomed to it, and secondly, it is shown by the figures themselves. For it is generally true, as we have seen, that not alone are the figures for E and F which average respectively 1.7 and 3.5 materially increased in G and H, but what is especially significant, so also are the figures for A and B increased in C and D, despite the fact that these average above 7. Hence the figures seem conclusively to indicate that the sound diminishes the weight sensation.[1] The subjects thought themselves to some extent able to verify the inhibiting effects of the sound when (after the year's work was over) they were told of it and special tests were repeated. Previously, however, they had not been able to guess the effect of the sound for themselves and similarly they were not able to notice that when sounds occurred in the hall or out-doors their judgments were affected thereby. *This indicates the interesting fact that one may be distracted and yet be unaware of it.*

That the diminution is not due to the memory but lies rather in the sensation process itself is shown by a simple calculation: In the cases of D and H, the sound occurs with the first weight which is remembered and thus compared

[1]Upon being questioned as to whether the sound made the concomitant pressure less 'clear,' three of the subjects replied that it did in some cases, but often or usually it did not seem to. In this connection it must be remembered that the sound was repeated again and again in the course of the experiment. Two of the subjects thought it possible or likely that the sound had a characteristic effect to reduce the clearness of the pressure sensation. The subjects were not in agreement in their opinions as to whether a reduction of clearness in the pressure sensation would lead them to judge the corresponding weight lighter than otherwise, some believing it might, others that it would not. However, if confidence in judgment is a function of the clearness of sensations judged, then it is pertinent to mention that for the most part, though not always, the subjects said that they had about the same degree of confidence in the truth of their judgements when the sound came as when it did not. Excepting, however, that the operator frequently noted not a decrease but rather a very marked increase in confidence as expressed in the tone of voice with which a given weight was judged heavier than a lighter one when the sound occurred with the latter.

with the second. But the total average increase from B to D is—as calculated from the figures just given—.5 while that from F to H is 1.1. Now in the cases of C and G the sound occurs with the second weight and here the subject often judges while the latter is still on his hand and hence memory in no way enters in as a significant factor. But the increases from A to C and from E to G are respectively .5 and 1.1, thus showing that the diminution of the sensation is not due to fading of the memory image.

The subjects remarked that they often judged heaviness by the distance which the rod *T* seemed to push down into the flesh. To the extent to which they did this they were estimating weights perhaps not simply by strengths of sensation but rather by qualitative characteristics of the sensations. But in the latter event one would reasonably expect that the sound would have no influence on their judgments of heaviness, excepting, perhaps, to render them less confident and constant. If, therefore, one finds—as we actually have—that it is in general true that a sound concomitant with a pressure tends to make the corresponding weight judged less heavy than otherwise, it seems warranted to conclude that *the sound diminishes the strength or intensity of the weight sensation.*[1] To be sure additional evidence is very welcome, and this is furnished by analogy in Series 2.

SERIES 2.

Inhibition of Sound Sensations by Pressure Sensations.

§5. *Apparatus.*—In this series it was undertaken to discover whether sound sensations of moderate strength can be inhibited by pressure sensations. The gravity phonometer is generally used to make sound sensations of various intensities, but it is very inferior to the tuning-fork which may

[1]The strength of the conscious sensation—the subjective intensity—is meant here, since the sound of course does not interfere with the objective stimulus. It is convenient to give the name of objective intensities to correspond proportionately with the objective stimuli, though these are essentially fictional, and not real entities, since the real intensity of a conscious sensation—as we seek here to demonstrate—depends not solely on the quantity of the objective stimulus, but decidedly upon subjective conditions as well.

be arranged as shown in the accompanying sketch (Fig. 2). An electrical tuning-fork of 256 vibrations was placed in an adjoining room and connected in single series with a key *C*, a rheostat *D* and a telephone receiver *G*, to which was attached a megaphone *H*. To the rheostat ('*G–R*' rheostat, with Gebr. Ruhstrat composition-wire resistance) was attached a millimeter scale along which the slider *E* moved, thus getting variation of the current that passed through the tele-

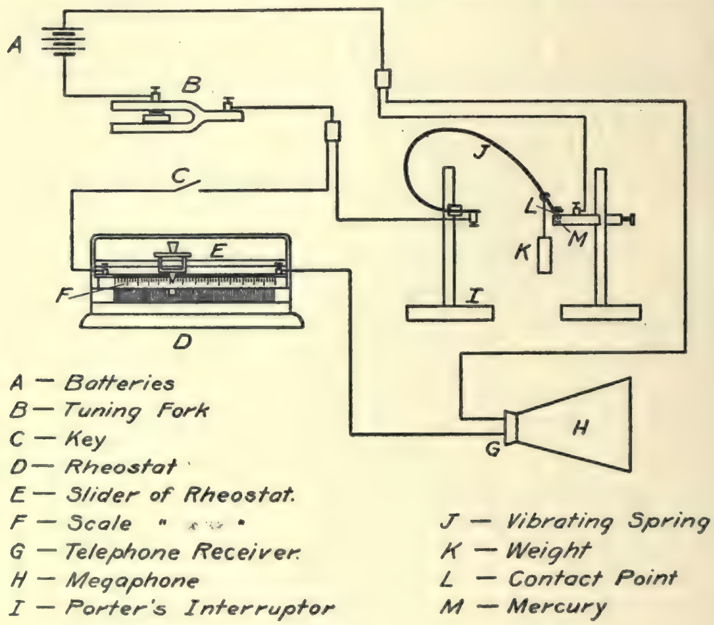


FIG. 2.

phone receiver and hence getting corresponding variation of the loudness of the sound.

If the apparatus consisted merely of the fork, rheostat, megaphone, key and connecting wires, one could not produce sounds of short duration at will, for it would be necessary to go into the other room to start up the tuning-fork each time a sound were desired. Now it is necessary so to arrange the apparatus that the tuning-fork is kept going all the time. To this end the leads from the tuning-fork were also connected up with wires leading through the apparatus marked *I*.

This instrument is known as 'Porter's Interrupter' and consists of a spring *J* which is bent over and held down by a weight *K*. The spring can be arranged with various vibrating lengths, but in the present experiment it was so arranged that it made a vibration in half a second. The contact point *L* was arranged so that it just touched the surface of the mercury *M*. Now when *K* was pulled down a short distance by the hand of the operator and then released, *L* would rise out of the mercury and thus break the circuit for one half second. With the key *C* closed and *L* in contact with *M* practically all the current would always pass through the wires that lead through *J*, because here the resistance was very much less than through *D*. But upon breaking contact with *M*, the current would be forced to go through *D* and *G*, thus making a sound one half second in duration, the relative intensity of which could be fixed at will by moving the slider *E*.[1]

For the pressure sensations the apparatus was used as shown in Fig. 3. The hand of the subject was placed as before with the cork disc upon it under *T*. A constant weight of 10 gr. was placed under the pan *R*, thus keeping *T* in contact with the cork. Throughout Series 2, the pans *R* and *S* each bore a weight of 300 gr. The one, however, causes pressure down on the point *B* and the other up, so that each neutralizes the other so far as any motion of the lever *A–B* is concerned. In order to apply a sudden pressure of 300 gr. to the cork on the subject's hand, the operator suddenly pressed down on the lever *E–F* at the point *F*. Since the pressure sensation should be simultaneous with the sound it was very important for the operator to press down on *F* with the left hand simultaneously with the commencement of the sound. The pressure on the hand of course outlasted the sound, for it was not possible under the conditions to make its duration only one half second. The essential thing, however, is to have the pressure commence with the sound and last at least as long.

[1]Of course what is actually secured is a satisfactory approximation to one half second rather than the precise attainment. It makes for accuracy to pull down *K* an equal distance each time.

§6. *Procedure.*—On the whole this was like that of Series 1, except that the signals 'One!' and 'Nine!' were omitted, the only signal used being the word 'ready' followed by the sound a little thereafter. The second sound occurred six seconds after the first. The subject was asked to judge intensities (*i. e.*, loudness) of sound and to reply 'fainter' or 'louder,' or 'same' with reference to the second sound. Slight qualitative differences in the sound did, it is true, enter in as an error, and the subject was warned against confusing these with quantitative differences and against associating qualitative peculiarities with any given intensity, so as to try to identify the latter thereby. For while a somewhat

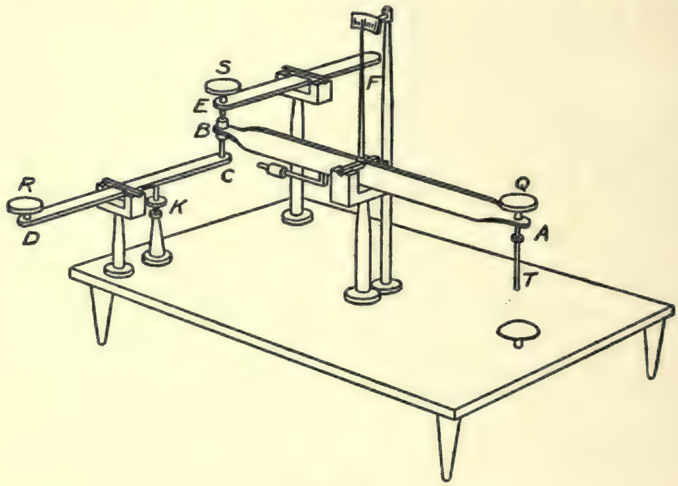


FIG. 3.

raucous tone, for instance, often characterized the stronger sounds, so also did it sometimes characterize the faint sounds, and hence it was explained to the subjects that they must not be misled thereby. Therefore, while the judgments doubtless were frequently influenced by qualitative differences, we are safe in believing that what was compared on the whole was essentially intensities of sounds.

At first the operator was in doubt as to what duration the sound should have. Various durations such as 3, 2 and 1 second were tried during the period when he was trying to

learn what was the requisite technique. Usually the practice results with these durations showed an inhibiting effect of the pressure on the sound, but often they did not. In order to obtain exact results the operator concluded that the proper duration of the sound should be one half second, firstly because this would largely preclude the possibility that the subject's attention should adapt itself to the pressure while the sound was still in operation, and secondly because it is possible that the pressure sensation quickly loses in strength.

After the subjects had been given considerable practice the operator determined what strengths of sound should be given to each of them. Because of personal differences in discrimination power, the same differences of sound could not be used for all subjects alike. Table III. indicates the relative strengths of sounds given together with the order in which

TABLE III.

	Subject 3.		Subject 4.		Subject 9.		Subject 5.	
	First Sound Given.	Second Sound Given.	First Sound Given.	Second Sound Given.	First Sound Given.	Second Sound Given.	First Sound Given.	Second Sound Given.
A	6.	7.	6.5	8.	6.2	7.4	8.7	9.4
B	7.	6.	8.	6.5	7.4	6.2	9.4	8.7
C	6.	7./300	6.5	8./300	6.2	7.4/300	8.7	9.4/300
D	7./300	6.	8./300	6.5	7.4/300	6.2	9.4/300	8.7
E	6.	5.	6.5	5.	6.2	5.	8.7	8.
F	5.	6.	5.	6.5	5.	6.2	8.	8.7
G	6.	5./300	6.5	5./300	6.2	5./300	8.7	8./300
H	5./300	6.	5./300	6.5	5./300	6.2	8./300	8.7

they occurred. The numbers stand for sounds varying with the positions of the slider E of the rhesotat and have no precise significance except that the smaller the number, the louder is the corresponding sound. The sign /300 indicates the application of a pressure of 300 gr. We shall once more use the symbol S for the standard sound (6. for Subj. 3), while C represents the fainter, and C' the louder sound compared with it.

§7. *Results.*—It will be seen that the results shown in Table IV. are of analogous nature to those of Series 1. The italic figures are larger than the corresponding roman ones in 29 out of 32 cases, and this means that a given sound (S) was

judged louder than another sound (C or C') when a simultaneous pressure occurred with the latter than when it did not. We are permitted to infer that *the conscious intensity of sound sensations may be reduced by concomitant pressure sensations*, a thing which harmonizes with and helps to confirm the results of Series 1.

These results are in agreement, moreover, with those of an experiment performed by Bentley, who used various sounds produced by means of the gravity phonometer with odors as distractives. He found that the odor caused the sound with which it was given to be 'underestimated,' and concluded that distraction may reduce the intensity of sensation.[1] Heymans, also, asserts that sensations above the limen may suffer a partial loss of intensity, saying that in his experiments when a given stimulus was able to produce a faint sensation despite the presence of another stronger sensation, then upon the removal of the latter the subject reported a perceptible increase of intensity. Other evidence, he claims, lies in the well known phenomena of intensity-contrast, and he undertakes special experiments in light and brightness contrast to show that this is so.[2] Finally there are the experiments of McDougall which show inhibition in the rôle of a reduction of psychic intensity. On page 185 of his 'Inhibitory Processes within the Nervous System' (*Brain*, 26, 1903) he speaks of 'sensation-intensity' as being 'subjectively diminished.' Again in a discussion of Fechner's paradox in connection with inhibition he speaks of 'loss of brightness,' which is of course loss of visual intensity.[3] And when in his series of articles on vision in *Mind*, 26, N. S., 10, he continually speaks of the 'fading' of images he is apparently speaking of loss of intensity. Hence there seems in general no room for doubt but that the subjective intensity of sensations may be reduced by the presence of other sensations.

[1]Bentley, PSYCHOL. BULLETIN, IV., 1907, 212 f. Referred to by Titchener *Psychol. of Feeling and Attention.*

[2]Heymans, *ibid.*, 41, 1906, 28 f. and 89 f.

[3]*Brit. Jour. of Psy.*, 1, 1904–05, 114–115.

TABLE IV.

		Subject 3.		Subject 4.		Subject 9.		Subject 5.	
Series 2–A.									
A	*C*	5½	*8½*	4½	*6½*	4	*10*	6½	*7½*
B	*D*	8	*10*	9½	*9½*	9	*10*	7½	*9½*
A+B	*C+D*	13½	*18½*	14	*16*	13	*20*	14	*17*
E	*G*	½	*1*	1	*2*	0	*5*	0	*4*
F	*H*	4½	*7*	4	*7½*	5	*9*	5	*6*
E+F	*G+H*	5	*8*	5	*9½*	5	*14*	5	*10*
A+B+E+F	*C+D+G+H*	18½	*26½*	19	*25½*	18	*34*	19	*27*
Series 2–B.									
A	*C*	7	*8*	0	*½*	2	*8*	5½	*9½*
B	*D*	9	*10*	2½	*7½*	8	*10*	7	*9*
A+B	*C+D*	16	*18*	2½	*8*	10	*18*	12½	*18½*
E	*G*	½	*0*	7	*9½*	1	*3*	3	*6*
F	*H*	4	*4*	9½	*10*	8	*9*	2½	*7½*
E+F	*G+H*	4½	*4*	16½	*19½*	9	*12*	5½	*13½*
A+B+E+F	*C+D+G+H*	20½	*22*	19	*27½*	19	*30*	18	*32*
Total sum	*Total sum*	39	*48½*	38	*53*	37	*64*	37	*59*

Figures opposite single letters in roman type (A or B or E or F) in each case represent the number of times a given sound (*S*) was judged louder than another one (*C* or *C′*); while in the space to the right of each such figure in roman is a corresponding figure in italic which indicates the number of times *S* was judged louder than *C* or *C′* when a pressure of 300 gr. was simultaneously applied to the right hand.

SERIES 3.

The Inhibitory Effect on Sound Sensations of Pressure Sensations of Different Intensities.

§8. *Apparatus.*—For sounds, same as in Series 2. For pressure sensations likewise, except that here weghts of 100 gr., 300 gr., and 500 gr. were used.

§9. *Procedure and Results.*—Only two subjects were used in this series (subjects 5 and 2). The subjoined table (V.) is practically self-explanatory. It shows, for instance, in the column headed 'Series 3–A' that the sound 5.7 was judged louder than 5 one time out of ten when there was no simultaneous pressure, one time out of ten when a pressure of 100 gr. occurred with 5, also one time with a pressure of 300 gr., and four times with a pressure of 500 gr. Thus the reader will be able easily to interpret the table. The observations are of course too few in number to permit quantitative formulations, but at least they suggest it as probable that *the greater*

the stimulus the stronger tends to be the inhibitory power of the corresponding sensation, a thing which one would naturaly presume to be true.

SERIES 4.

The Inhibitory Effect of Inattention on Pressure Sensations.

§10. *Introduction.*—Hereupon a very interesting problem develops itself. It is apparently a very fundamental relation of mind if when two sensations are simultaneously presented, the one may inhibit the other. Now what part, if any, does attention play in all this? May we assume, as would be quite natural, that what the inhibitor does is substantially to dis-

TABLE V.[1]

Subject.		First Sound Given.	Second Sound Given.	Number of Times Out of Ten First Sound Judged Louder.				Total.
				Series 3–A.	Series 3–B.	Series 3–C.	Series 3–D.	
5	A	5.7	5.	1	1	2½		4½
	B	5.7	5./100	1	3	2		6
	C	5.7	5./300	1	1	1		3
	D	5.7	5./500	4	3½	2½		10
2	A	6.	5.	1½	1½	0	1	4
	B	6.	5./100	1½	1	1	2	5½
	C	6.	5./300	3½	2	1	½	7
	D	6.	5./500	6	2	2	2	12
		First Sound Given.	Second Sound Given.	Number of Times Out of Ten Second Sound Judged Louder.				Total.
				Series 3–A.	Series 3–B.	Series 3–C.	Series 3–D.	
5	E	5.5	6.	3½	5	5		13½
	F	5.5/100	6.	8	7½	3		18½
	G	5.5/300	6.	5	7	6		18
	H	5.5/500	6.	8	8½	5½		22
2	E	5.3	6.	2½	5	5	4½	17
	F	5.3/100	6.	7½	6	4½	3½	21½
	G	5.3/300	6.	8	6½	7½	3	25
	H	5.3/500	6.	8	6½	9	8½	32

tract the attention from the inhibited? It seems inviting not to asume this, but rather to institute a new inquiry whether it is so. Thus we have encountered the old and difficult question whether increased attention brings increased intensity to sensation. But in certain respects we have unusual facili-

[1]Refer to p. 40.

ties for its solution, in as much as we are already supplied by the foregoing series with very efficient distractives together with the correct means to use them. The way is, namely, so to arrange the conditions that the distractive begins with the sensation distracted from. The importance of this—which I believe is not generally understood—deserves special emphasis. For if the the inception of the distractive is before or long after that of the sensation to be distracted from, it is likely to lose much of its inhibitory power, and it is even possible—so far as we at present know—that it may under certain conditions augment instead of inhibit. It is imperative, moreover, that the sensation distracted from be not of too long duration. Else once more the distractive—by virtue of a principle which we shall later develop—may lose its initial inhibitory influence and eventually indirectly produce an augmentation. Doubtless here lies the source of the conflicting reports well known to the literature about distractives.

Hence we need to make the time-periods short as well as simultaneous. One more special condition needs to be fulfilled if we are to develop a precise means of treatment of the problem of voluntary attention and intensity. This condition requires that contrary to the usual methods, the distractive must not be absent in one set of observations and present in the other set which is to be compared therewith, but is rather to be present in both sets, the important point of difference being that in one set the attention is voluntarily put on the distractive, while in the other it is not. Thus we will be permitted to abstract from the influence of the distractive as such, and may learn what are the differences produced by the voluntary attention alone. This passage will become clearer after we have presented and explained our tables.

§11. *Apparatus and Procedure.*—The apparatus was the same as that of Series 1, pressure sensations of four seconds in duration being produced and judged. The principle of procedure was to select a convenient weight *S*, together with a series of five other weights and to make two classes of observations with the purpose of comparing their results: first, to find out how many times out of ten the *S* weight—

always given first—would be judged heavier than each of these weights if the sound always occurred with the application of the latter while the attention was allowed naturally to fall on the pressures which were to be compared; and second, to find out how many times *S* would be judged heavier than each of these weights under the same conditions as before except that the attention were kept on the sound during its occurrence, but after its cessation transferred to the right hand, so that the sensation from the *S* weight was always received with normal or undistracted attention. In Table VI., which shows what weights were used, the capital letters A, B, C, D, E, serve to designate the couplets of weights compared with the attention resting naturally on the weight

TABLE VI.

	Subject 6.		Subject 7.		Subject 8.		Subject 9.	
	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).
A	18/*n*	20	12/*n*	15	10/*n*	12	10/*n*	12
B	20/*n*	20	15/*n*	15	12/*n*	12	13/*n*	12
C	23/*n*	20	18/*n*	15	13/*n*	12	16/*n*	12
D	25/*n*	20	21/*n*	15	15/*n*	12	19/*n*	12
E	28/*n*	20	24/*n*	15	18/*n*	12	22/*n*	12
a	18/*n*	20	12/*n*	15	10/*n*	12	10/*n*	12
b	20/*n*	20	15/*n*	15	12/*n*	12	13/*n*	12
c	23/*n*	20	18/*n*	15	13/*n*	12	16/*n*	12
d	25/*n*	20	21/*n*	15	15/*n*	12	19/*n*	12
e	28/*n*	20	24/*n*	15	18/*n*	12	22/*n*	12

sensations, while the small letters, a, b, c, d, e, designate the couplets in which the attention was voluntarily concentrated on the sound. The sign /*n* stands for the sound.

Of course practice was necessary before the subjects learned to concentrate on the sound (*i. e.*, in the cases of the small letters). They were told that in case such concentration made them unable to judge with confidence, they should mention the fact and withhold their reports if necessary. Their experience was, however, that when the concentration was directed to the sound and the concomitant pressure therefore less strongly attended, there nevertheless remained a good memory image of that pressure which enabled them to judge it in a very satisfactory way.

Subjects 6 and 7 seemed to acquire the needed control of the attention with great readiness. Subject 8 usually reported the control good, but the operator frequently had doubts about the matter.[1] Subject 9 admitted that he was easily distractible by noises and the like occurring while he was studying, and he sometimes stated that his attention to the sound was not good. It was necessary to give emphasized instructions not merely to attend to the sound during the first pressure application but in all cases to return to the second pressure with normal attention, else there was danger that the second weight would be insufficiently attended. In order to stimulate the subjects to proper effort the operator reminded them very frequently (sometimes each time) what was to be done—if only with a word.

In taking the protocol ten capital letters were first given and then ten small letters or *vice versa*, care being taken to equalize conditions. That is to say, ten judgments were passed in succession with the attention voluntarily directed to the sound in each case, and next ten with the attention not on the sound but naturally directed toward the pressures on the right hand which were to be discriminated. Judgment was always passed in terms of the second pressure, and the numbers in Table VII. represent the number of times the second weight was judged heavier.

§12. *Results.*—It will be noticed that the results are very pronounced indeed. The italic numbers show a great increase over the roman. That is to say, the number of times a given weight is judged heavier than another is much increased if the attention is not directed toward the latter. *Decrease of attention to a pressure seems therefore to involve decrease of intensity of sensation.*[2]

[1]At such times on several occasions he stopped taking the records in order to give the subject special practice, a thing also done with subject 9.

[2]Subject 3 also was tried for this series. This subject, however, found the rattling sound of the buzzer very disagreeable and apparently was not able to perform the desired concentration. Hence only 3–A was performed with this subject. It is interesting to state that here $A+B+C+D+E=30$ while $a+b+c+d+e=23\frac{1}{2}$, showing a decrease where with proper attention there presumably would have been an increase. The instruction to concentrate on the sound, instead of being carried out, led to the opposite achievement.

Therefore we are warranted in inferring that in Series 1 and 2 the diminution of the one sensation by another was identical with a distraction of the attention. Or rather, it is more illuminating to put it the other way: *in the case of simultaneous sensations, distraction of the attention is equivalent to diminution or inhibition of one by the other.* Also, some of the subjects reported that attention away from the pressure seemed to give its sensation a changed quality. We may perhaps explain this by remembering the principle elaborated by Heymans that faint sensations may be utterly suppressed

TABLE VII.

		Subject 6.		Subject 7.		Subject 8.		Subject 9.	
Series 3–A.									
A	*a*	9	*9*	8	*8½*	6	*7*	4	*7*
B	*b*	5	*7*	5½	*6*	4½	*4½*	6	*3*
C	*c*	3	*10*	6½	*10*	4	*2*	4	*7*
D	*d*	4	*8*	6	*8*	7	*4*	4	*4*
E	*e*	3	*5*	5	*7½*	4	*3½*	2	*4*
Sum	*Sum*	24	*39*	31	*40*	25½	*21*	20	*25*
Series 3–B.									
A	*a*	7	*10*	8½	*7*	8	*7*	6	*4*
B	*b*	9	*8*	3	*7*	5	*8*	3	*5*
C	*c*	8	*9*	5½	*9*	4	*9*	2	*3*
D	*d*	5	*8*	5	*8*	3	*6*	1	*1*
E	*e*	5	*2*	2	*7*	2	*2*	3	*3*
Sum	*Sum*	34	*37*	24	*38*	22	*32*	15	*16*
Series 3–C.									
A	*a*	7	*8*	9	*9*	9	*8*	7	*7*
B	*b*	5	*9*	6½	*7½*	4	*6*	5	*7*
C	*c*	8	*8*	5½	*8½*	5½	*9*	2	*3*
D	*d*	7	*7*	4	*5½*	5	*6*	4	*3*
E	*e*	3	*5*	1	*5*	3½	*4½*	0	*2*
Sum	*Sum*	30	*37*	26	*35½*	27	*33½*	18	*22*
Series 3–D.									
A	*a*	7	*7*	7	*6½*	9½	*6*	7	*6*
B	*b*	9	*10*	5½	*9*	4	*5*	3	*5*
C	*c*	5	*7*	4	*7*	6	*6*	5	*7*
D	*d*	5	*6*	5½	*8½*	5½	*6*	3	*2*
E	*e*	2	*7*	4½	*7*	3	*6*	2	*2*
Sum	*Sum*	28	*37*	26½	*38*	28	*29*	20	*22*
Total sum	*Total sum*	116	*150*	107½	*151½*	102½	*115½*	73	*85*

Each figure in roman type opposite A or B or C or D or E represents the number of times out of ten that a certain weight *S* was judged heavier than some other one under conditions described in the context. In the space to the right of each such figure in roman is one in italic which represents the number of times out of ten that *S* was judged heavier than that same other weight under the same conditions as before except that the latter weight received less attention.

by other strong ones. Now our pressure sensation was not a simple sensation, but rather a group of sensations—some of different qualities and strengths—some of light surface contact, others of deeper pressure. Therefore when one subject reports that withdrawing the attention from the pressure makes the sensation become 'dull and heavy,' we may reasonably suppose that this is due to the fact that the fainter sensations become suppressed, leaving the stronger ones as the residue and thus changing the character of the whole. It is interesting to think that increasing the attention of such a complex sensation is like increasing the volume of a clang, for thereby one changes not alone its quantity but its quality by making the 'overtone' sensations, so to call them, become more and more perceptible.

Series 5.

The Inhibitory Effect of Inattention on Sound Sensations.

§13. *Apparatus and Procedure.*—This is in principle the same as 3, but instead of judgments about pressure sensations with sounds as distractives, the judgments were made concerning sounds with pressures for distractives. The same instruments for pressures and sounds were used as those of Series 2, and the sounds here were also one half second in duration. Of course the operator made every effort to observe the caution that the pressure be applied simultaneously with the beginning of the sound. Table VIII. shows the relative strengths of sounds used, and the sign /300 indicates that a pressure of 300 gr. was applied with the first sound.

Table VIII.

	Subject 6.		Subject 7.		Subject 8.	
	First Sound Given.	Second Sound Given.	First Sound Given.	Second Sound Given.	First Sound Given.	Second Sound Given.
A	5. /300	8.	5./300	7.	6./300	8.5
B	6. /300	8.	6./300	7.	7./300	8.5
C	6.7/300	8.	7./300	7.	8./300	8.5
D	8. /300	8.	8./300	7.	9./300	8.5
a	5. /300	8.	5./300	7.	6./300	8.5
b	6. /300	8.	6./300	7.	7./300	8.5
c	6.7/300	8.	7./300	7.	8./300	8.5
d	8. /300	8.	8./300	7.	9./300	8.5

The numbers stand for strengths of sounds: the smaller the number the louder the sound. The sign (/300) stands for the application of the 300 gr. pressure. In the couplets of sounds designated by the capital letters the attention rests naturally on the sounds to be discriminated. In those designated by the small letters the first sound receives less attention than in the case of the capital letters for it is voluntarily placed on the pressure sensation; but the second sound receives normal attention.

§14. *Results.*—The numbers in Table IX. represent the number of times out of ten that the second sound was judged louder. The results are similar to and in harmony with those of Series 4. The italic figures are in 19 out of 24 cases

TABLE IX.

		Subject 6.		Subject 7.		Subject 8.	
Series 5–A.							
A	*a*	0	*4*	1	*4*	1½	*5*
B	*b*	1	*4½*	2½	*8*	5	*6*
C	*c*	5	*6½*	4½	*9*	8	*9*
D	*d*	7½	*6½*	10	*9*	8	*9½*
Sum	*Sum*	13½	*21½*	18	*30*	22½	*29½*
Series 5–B.							
A	*a*	2	*2½*	2	*3½*	1½	*3½*
B	*b*	2½	*6½*	1	*8*	7	*7*
C	*c*	7½	*7*	7½	*10*	8	*7*
D	*d*	6½	*9*	9	*10*	9	*9½*
Sum	*Sum*	18½	*25*	19½	*31½*	25½	*27*
Total sum	*Total sum*	32	*46½*	37½	*61½*	48	*56½*

Each figure in roman type opposite A or B or C or D represents the number of times out of ten that a certain sound *S* was judged louder than some other one under conditions described in the context. In the space to the right of each such figure in roman is one in italic which represents the number of times out of ten that *S* was judged louder than that same other sound under the same conditions as before except that the latter sound received less attention.

larger than the roman, which means that a sound is judged less loud when simultaneously the attention is voluntarily directed to something else. If these results be accepted then without doubt it is true that *decrease of attention involves decrease of intensity of sound.*[1]

If so, then we have in this principle the means to explain the well-known phenomenon called the 'temporal error,' namely, that in comparing two successive sensations the

[1]Heymans, also, states that attention to an inhibiting psychic process increases its inhibitory power. *Ibid.*, 53, 1909, 402.

second usually is overestimated. It is obviously due to this that after the subject receives the first stimulus he awaits the second with stronger expectant attention.[1] Similarly, we can readily understand from the principles developed in the above four series why it is true that small differences between weights are much harder to discriminate when simultaneously than when successively given. The sensations from simultaneously given weights inhibit each other and thus forestall anything like the precise discrimination possible with successive sensations.

Series 6.

The Power of Voluntary Attention to Overcome Inhibition.

§15. *Procedure.*—Since in Series 1 and 2 it had been shown that a sound or pressure sensation may inhibit a pressure or sound sensation it suggests itself to inquire whether by an extraordinary effort to concentrate on the sensation judged the inhibiting influences of the distractor might not be neutralized. One set of observations (80) was taken with subject 7 and two sets with subject 9, sounds being judged. More would have been taken but for derangements in the apparatus and batteries. The following table shows what was done in the case of subject 7.

	Sound First Given.	Second Sound.
A	5	6.7
B	6	6.7
C	7	6.7
D	8	6.7
a	5/300	6.7
b	6/300	6.7
c	7/300	6.7
d	8/300	6.7

The sign /300 indicates when a 300-gr. weight was given with the first sound. The subject was directed to concentrate very hard on the sound that was accompanied by the pressure.

§16. *Results.*—The results for 80 observations with subject 7 were: A+B+C+D=17 and a+b+c+d=14. These

[1] Sometimes, however, subjective conditions are such as to bring heavier attention and corresponding overestimation to the first stimulus, but this is seldom found to occur for a large number of times in succession.

numbers represent the number of times the second sound was judged the louder. For subject 9 the results for 2 sets of 80 observations each were respectively $A+B+C+D=26$ while $a+b+c+d=27$ and $A+B+C+D=24$ while $a+b+c+d=18.5$.

These figures mean that the presence of the pressure simultaneous with the first sound did not succeed in causing the second sound to be judged louder more times than when the pressure was absent. In other words the extra concentration on the sound sensation prevented it being diminished by the pressure sensation.

This same compensation was demonstrated by some few observations on pressure sensations with sounds as distractives. 100 judgments by subject 8 and $A+B+C+D+E=16.5$ while $a+b+c+d+e=14.5$. A single test made with subject 7 also brings out the augmentor effect of voluntary attention. For this subject we take Series 3–C together with 3–D in Table VII. the figures $A+B+C+D+E=52.5$. This represents the number of times a pressure sensation was judged heavier than others when the sound of the buzzer occurred with the latter. We found that this number would be increased if the attention was voluntarily directed to the sound. Now a hundred tests were made with the direction to concentrate hard on the first pressure, with which the sound came, and the results were $a+b+c+d+e=35.5$. In short, voluntary concentration on the first pressure decreases the number of times that the second pressure is judged heavier.

Hence the results indicate that *the influence of an inhibiting agency may be neutralized by the voluntary attention.* It seems perfectly permissible to infer that the same thing could under favorable conditions be done by the involuntary attention, and that in general it it true that *just as decrease of attention involves decrease of intensity of a sensation, so conversely does voluntary or involuntary concentration involve increase of intensity.*

SERIES 6.

Inhibition of Pressure Sensations by Other Stronger Pressure Sensations.

§17. *Apparatus and Procedure.*—The writer had commenced his investigations on the inhibition of sensations with a series in which was tested the effect of lowering a 200-gr. weight over a pulley onto the left hand upon the strength of other pressure sensations on the right. The results were not the same for all the subjects, seeming to show an inhibition of the sensations of the right hand for some subjects and an augmentation for others. At that time, however, he did not understand the necessity of keeping the time periods of the inhibiting sensation and of the inhibited sensation in a certain constant relation. Hence it seemed desirable to repeat the test under more exact conditions.

The pressure sensations judged were produced on the right hand with the same apparatus as in Series 1 and the method was the same, except that the pressures lasted only two seconds instead of four. A special arangement was used for pressures on the left hand as shown in Fig. 4. The subject's left hand was placed palm downwards on the rest *N* and a cork disc again of about 15 mm. in diameter placed upon it. Now the screw *M* was turned down until the pin under *S* was in light contact with the cork. The pan *S* was loaded with a 300- or a 500-gr. weight. The fingers of the operator continually pressed down the lever *E–F* at the point *F*, so that he had but to remove his fingers in order that the

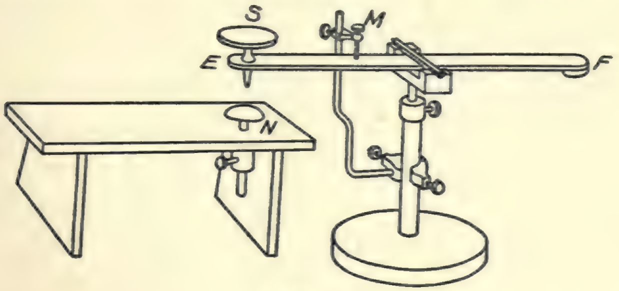


FIG. 4.

300- or 500-gr. pressure might be applied to the subject's hand.

Great effort was made to apply the pressures to both hands as nearly simultaneously as possible, and though the subjects asserted that they were often apperceived as not simultaneous, we may nevertheless reasonably assume that the success was in the long run satisfactory. Table X. shows the weights used for each subject.

TABLE X.

	Subject 3.		Subject 8.		Subject 1.		Subject 4.	
	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).
A	22	20	18	15	18	15	21	17
B	20	22	15	18	15	18	17	21
C	22	20/300	18	15/500	18	15/500	21	17/500
D	20/300	22	15/500	18	15/500	18	17/500	21
E	22	24	18	21	18	22	21	26
F	24	22	21	18	22	18	26	21
G	22	24/300	18	21/500	18	22/500	21	26/500
H	24/300	22	21/500	18	22/500	18	26/500	21

The signs (/300) and (/500) indicate the application of pressure of 300 or 500 gr. respectively. Otherwise the explanation of this table is the same as that of Table I.

§18. *Results.*—The results for subjects 7, 8 and 1 show that the 500-gr. pressure did have a predominantly inhibitory effect on the sensations of the right hand. With subjects 3 and 8 this effect was clear from the first. With subject 1 some observations were previously taken where left-hand pressure 200 gr. and at another time left-hand pressure 400 gr. The results were inconclusive. This seemed possibly explanable because subject 1 made such restless efforts to judge the weights with the greatest accuracy and hence perhaps by frequent concentration on the right hand neutralized the inhibiting influence of the pressure on the left. Presuming that this might be the explanation of the case, the operator concluded that it would be necessary to have the weight on the right hand sufficiently heavy to manifest its influence in spite of the restless shiftings of the attention, and therefore he selected the 500-gr. weight. The results then showed, as previously remarked, that this weight had a marked inhibitory influence on the right-hand sensations.

As to the explanation of the results of subject 4 the writer is somewhat in doubt. That subject expressed a great lack of confidence in his judgment in this series, and the operator had similar misgivings. It is possible that the

TABLE XI.

		Subject 3.		Subject 8.		Subject 1.		Subject 4.	
Series 6–A.									
A	*C*	7	*10*	5	*7½*	8	*9*	3½	*0*
B	*D*	8	*8*	9	*10*	9	*8½*	7	*3½*
A+B	*C+D*	15	*18*	14	*17½*	17	*17½*	10½	*3½*
E	*G*	5	*10*	1	*6*	3	*6*	0	*0*
F	*H*	5	*6*	6½	*7*	½	*4*	2½	*1*
E+F	*G+H*	10	*16*	7½	*13*	3½	*10*	2½	*1*
A+B+E+F	*C+D+G+H*	25	*34*	21½	*30½*	20½	*27½*	13	*4½*
Series 6–B.									
A	*C*	5	*7*	3½	*9*	6½	*6½*	6	*4*
B	*D*	8	*8*	9½	*7*	7	*5*	8	*4½*
A+B	*C+D*	13	*15*	13	*16*	13½	*11½*	14	*8½*
E	*G*	2	*8*	1½	*10*	2	*6½*	1	*0*
F	*H*	3	*8*	3½	*7*	3	*3*	5½	*7½*
E+F	*G+H*	5	*16*	5	*17*	5	*9½*	6½	*7½*
A+B+E+F	*C+D+G+H*	18	*31*	18	*33*	18½	*21*	20½	*16*
Total sum	*Total sum*	43	*65*	39½	*63½*	39	*48½*	33½	*20½*

The explanation of this table is the same as that of Table II., except that the distractive used here is a simultaneous pressure of 300 or 500 gr., on the left hand instead of a sound.

strong pressure on the left hand exercised something of the same inhibitory influence in his case that it did with the others, for he reported that when the sensations from the right hand were faint they were sometimes almost crowded out of consciousness by the left-hand pressure. The subject stated that the pressure on the left hand markedly reduced the clearness of the sensations of the right hand. Upon being told of the results he thought them probably due to this unclearness coupled to an associated notion of heaviness whenever the left-hand pressure came. Other explanations, also, would be conceivable, but they only would introduce speculation.

At any rate so much was found to be true in this series: *that the intensity of the pressure sensations of moderate strength*

may often be diminished by other stronger pressure sensations.[1]

§19. *Conclusion.*—The results of our experiments have been to show that sound sensations of moderate intensity may be inhibited by simultaneous strong pressure sensations, that pressure sensations of moderate intensity may be inhibited by other simultaneous sensations either of sound or of pressure, that increase of attention to the inhibitor increases its effect, while increase of attention to the other sensation decreases the inhibition. This means that in the case of simultaneous sensations what is commonly known as effective distraction of the attention consists of the inhibitory influence which one sensation exerts upon another; and that degree of (subjective) intensity of sensation is a function of degree of attention. And though Heymans does not take up any special discussion of this last proposition, it can be deduced also from the facts he develops and demonstrates: a sensation to which there is adequate stimulus rises above the threshold in proportion as inhibition of it by other simultaneous sensations is absent. In other words, its intensity increases in proportion as the absence of other sensations permit the stimulus to be more and more attended. In short, the experiments of Heymans and Bentley taken together with our own seem to prove that degree of intensity of sensation is a function not of amount of objective stimulus alone, but of the latter taken together with the influence of other simultaneous sensations.

[1]We need to record in passing that questions about 'clearness' brought no unanimity of answer from the subjects, as previously stated; but it is interesting to note that in Series 4 and 5 where the attention was voluntarily placed on the inhibitor and the intensity of the other sensation much reduced as shown by the reports, yet the two subjects (6 and 7) who had the greatest skill in thus placing their attention agreed that for the most part the sensation not attended to was practically as clear as that upon which attention was directly placed. It was not our attempt, however, to make systematic and exhaustive investigation with reference to clearness and attention; but it seems well to throw out the suggestion here that our conclusion that decrease of attention brings decrease of intensity to sensation does not necessarily conflict with such facts as that distraction by noise during the process of addition brings decrease of clearness to the mathematical material attended to. (Geissler, 'The Measurement of Attention,' *Amer. Jour. of Psy.*, 20, 1909.)

No. 5 208

[Reprinted from the PSYCHOLOGICAL BULLETIN, Vol. VII, No. 6, June 15, 1910.]

SUPPRESSION WITH NEGATIVE INSTRUCTION.

BY HERBERT SIDNEY LANGFELD,
Harvard University.

A series of experiments is in progress in the Harvard Psychological Laboratory which aims to compare the association and the reproduction of normal subjects under normal conditions with those of normal subjects under distraction, and with those of abnormal subjects. One of the features of the experiment is the requirement of a suppression. As the tests on the normal subjects without distraction are already completed, and throw some light on the nature and development of the suppression process, the results, which bear upon this process, will be set forth in the present paper.

The experiment was conducted in the sound-proof room of the laboratory. Visual stimuli, consisting of small pictures in black and white of simple objects such as animals, vegetables, utensils, etc., were employed. There was never more than one object or several of the same kind of objects in the same picture, except occasionally an inconspicuous background. The pictures were mounted on black cardboard, which could be slipped into grooves behind a small square window cut in a black upright board. A black shutter, which was worked by hand, concealed the picture. A lamp with a reflector illuminated the window of the instrument; the rest of the room was in semi-darkness. The subject was seated directly behind the lamp and about one and one half feet from the picture.

The instructions for the A-tests were: "Shortly after you hear the word 'now' a picture will be exposed in the square opening. You are to speak the first word suggested to you by the object in the picture, unless it is the name of this object. You are not to name the object, but you may describe it or name any of its parts. For example, if it is a cow you may say small, old, head, etc. After the word is spoken you are to give the results of a careful introspection. Pay particular attention to the processes of suppression and association and to the imagery."

Ten pictures were shown and then the R-tests were begun, the pictures being given in the same order as before. The instructions were: "You will see the same pictures again. Try to give the same

association you gave before, but if another word should come up, you are not to inhibit it. As before you are not to name the object."

The time both for the A- and the R-tests was taken with a stop-watch. This was measured from the opening of the shutter to the first sound of the spoken word. There were sixty pictures in all, so that the experiment extended over six hours. With a few exceptions a week intervened between each series of ten pictures. Eight subjects took part in the experiment, all teachers or students in the department. They will be designated as A, B, C, D, E, F, G, H.

Subject A.

A = association test, R = reproduction test, R.T. = reaction time, k' = any kinæsthetic imagery, k'' = kinæsthetic imagery of the name of the picture, v = visual imagery, a = auditory imagery, S.S. = successful suppression, S.R. = successful repetition.

	A.		R.		A.				R.				A.	R.[1]
No.	R. T.	m. v.	R. T.	m. v.	k'.	k''.	v.	a.	k'.	k''.	v.	a.	S. S.	S. R.
1	1.2	.44	1.3	.21	10	8	4		4	3	2		10	6
2	1.3	.24	1.2	.22	5	5	2		4	2			9	5
3	1.2	.24	1.3	.6	7	3	2		11	4			8	5
4	1.7	.45	1.1	.19	7	5	4		3	2			10	7
5[2]	1.1	.18	1.	.24	5	4	0		3	2			8	7
6	1.1	.14	.8	.14	4	3	1		2	2			8	8
Av.	1.2	.28	1.11	.25	Per cent. of successful tests :								91%	65%

[1] The S.R. and the imagery other than k'' will not be discussed in this paper.

[2] No. 5 and 6, unless otherwise marked, contain only nine reactions.

Subject B.

	A.		R.		A.				R.				A.	R.
No.	R. T.	m. v.	R. T.	m. v.	k'.	k''.	v.	a.	k'.	k''.	v.	a.	S. S.	S. R.
1	2.1	.27			6	5	1	4					10	
2	1.3	.18			7	6							10	
3	1.1	.15	1.1	.14	10	10			4	3			10	7
4	1.1	.14	1.1	.24	9	8			5	5			9	6
5	1.3	.23	1.1	.21	6	5			4	4			9	8
6	1.4	.26	1.3	.21	14	4	1		7	4			9	6
Av.	1.38	.2	1.15	.2	Per cent. of successful tests :								98%	71%

Subject C.

No.	A.		R.		A.				R.				A.	R.
	R. T.	m. v.	R. T.	m. v.	k′.	k″.	v.	a.	k′.	k″.	v.	a.	S. S.	S. R.
1	1.7	.38	1.6	.34	5	4	2		3	0	1		9	5
2	1.7	.28	1.3	.30	9	7	2		4	3	0		10	7
3	1.7	.26	1.5	.44	5	3	1		2	1	1		10	8
4	1.5[2]	.32	1.3	.36	2	2	0		1	0	1		8	5
5	1.1	.25	1.1	.45	2	2	2		1	1			9	9
6[1]	.9[3]	.18	1.3	.36	3	3	0		1	0			8	4
Av.	1.43	.28	1.35	.37	Per cent. of successful tests:								96%	68%

[1] A month elapsed between No. 5 and 6.

[2] Contains only nine reactions.

[3] Contains only eight reactions.

Subject D.

No.	A.		R.		A.				R.				A.	R.
	R. T.	m. v.	R. T.	m. v.	k′.	k″.	v.	a.	k′.	k″.	v.	a.	S. S.	S. R.
1	1.6	.26	1.5	.23	8	5	2		1	1			10	4
2	1.3	.18	1.2	.24	7	7	2		2	2			10	6
3	1.4	.32	1.2	.24	5	5			1	1			10	4
4	1.3	.16	1.0	.12	1	1			3	3			9	9
5	1.1	.19	.9	.1	1	1	1		1	0			8	9
6	1.2	.16	1.1	.16	2	2			0	0			9	5
Av.	1.31	.21	1.15	.18	Per cent. of successful tests:								96%	64%

Subject E.

No.	A.		R.		A.				R.				A.	R.
	R. T.	m. v.	R. T.	m. v.	k′.	k″.	v.	a.	k′.	k″.	v.	a.	S. S.	S. R.
1	1.9	.56			1	5	1	3					10	
2	1.4[1]	.16	1.3	.26	3	3			4	4	1		7	8
3	1.5	.28	1.3	.24	3	3	3		2	2	1		10	7
4	1.6	.20	1.2	.16	4	4	2		2	2	1		10	8
5	1.3	.20	1.0	.08	10	9	1		7	6			8	7
6	1.4	.31	1.3	.39	4	4			6	5			9	9
Av.	1.51	.28	1.22	.23	Per cent. of successful tests:								95%	83%

[1] Contains only nine reactions.

Subject F.

No.	A.		R.		A.				R.				A.	R.
	R.T.	m.v.	R.T.	m.v.	k′.	k″.	v.	a.	k′.	k″.	v.	a.	S.S.	S.R.
1	1.5	.26	1.3	.26	10	5			8	3			8	7
2	2.0	.72	1.7	.37	6	5	4		15	7	5		8	5
3	1.5	.82	1.4	.34	7	5	1		8	7			7	7
4	1.5	.56	1.3	.30	15	6	1		11	4		1	9	9
5	1.6	.54	1.1[1]	.20	11	9	2		7	4			8	5
6	1.6	.58	1.1	.21	11	7			10	7			9	9
Av.	1.6	.58	1.31	.26	Per cent. of successful tests:								84%	74%

[1] Contains only eight reactions.

Subject G.

	A		R.		A.				R.				A.	R.
No.	R.T.	m.v.	R.T.	m.v.	k′.	k″.	v.	a.	k′	k″.	v.	a.	S.S.	S.R.
1	2.5	.66					1						10	
2	1.8	.20											10	
3	1.6	.32	1.1	.26			2						9	10
4	1.7	.44	1.1	.23									9	9
5	1.4	.22	.9	.10	1		1				1		9	9
6	1.3	.32	1.0	.24									9	8
Av.	1.71	.36	1.02	.21	Per cent. of successful tests:								96%	95

Subject H.

	A.		R.		A.				R.				A.	R
No.	R.T.	m.v.	R.T.	m.v.	k′.	k″.	v.	a.	k′.	k″.	v.	a.	S.S.	S.R.
1	1.7	.41	1.2	.18			1						8	6
2	1.6	.18	1.3	.16			1				3		10	7
3	1.7	.22	1.6	.32			4				3		10	6
4	1.4	.20	1.3	.22			7				5		10	8
5	1.5	.16	1.1	.12			2				2		8	7
6	1.6	.36	1.1	.10			2				1		9	6
Av.	1.58	.25	1.26	.18	Per cent. of successful tests:								95%	69%

An examination of the figures of the column under successful suppression (S.S.), which give the number of times the subject refrained from reacting with the name of the object, shows that in very few instances was the negative instruction not followed. The percentage of failures for the sixty tests range from 2 per cent. to 9 per cent. in the entire series, except in the case of F, who shows 16 per cent. This subject differs from the others also as regards the m.v., which is much greater and it is possible, judging from these facts and from

observation, that his attention, either from lack of interest or from fatigue, was not the best he could give.

As the actual inhibition of the spoken word is practically perfect from the beginning, in order to study the development of the process, we shall have to examine the tendency to name the object as revealed by the introspection on the kinæsthetic imagery. The question has been left open as to whether at any given time the subject means kinæsthetic imagery of the articulatory process or actual sensations in the organs of speech. So near the border of image and sensation, a judgment as to which is being experienced would only have value after a very long and special study by means of careful introspection. In column 6, under kinæsthetic imagery (k′), are tabulated all instances of such imagery whether of the name of the object or of an associated word. In column 7 (k″) are only those instances of the imagery concerned with the name of the object. It is this column which is of interest to us at present. In four of the six tables, which show kinæsthetic imagery, the amount decreases more or less gradually. This is proof of an increased power of suppression with practice. F is again an exception. Both he and E show a decided increase. Individual differences are to be expected. It is quite possible that they are of such a strong kinæsthetic type that only very long practice would overcome this tendency.

The figures thus far examined show the change from week to week. If we turn now to a comparison of the amount of kinæsthetic imagery in the A-tests with that of the R-tests on the same day, we find in all but F (and G and H) a decided decrease in the latter tests. Here there is shown the effect of practice in suppressing the same word. The fact that the interval between the two acts of suppression (in the A- and R-tests) is short and that a definite reaction word has been partially established in the A-tests also helps the inhibition.

The only datum that H's introspection reveals is that he is of a decidedly visual type, which may account for the entire absence of kinæsthetic imagery. G adopted a more definite attitude toward the experiment than did the other subjects, which, without doubt, helped the suppression. We will return to a description of this attitude in the following account of the process of suppression.

We shall first select examples from the records of the introspection illustrating the different forms of suppression and then describe the general development as deduced from the entire data. The examples are from the A-tests.

I. *Comb . . . Hair.* "I recognized comb. Kinæsthetic image

of comb. Locked the muscles of the throat, after thinking that I must not say the word. Then hair came."

Sleigh . . . Ride. "Recog. sleigh. Kin. image of sleigh. Thought I should not say it. Locked muscles and then said ride."

II. *Pump . . . Water.* "Recog. pump. Searched for word. Kin. image of pump. Thought that I must not say it."

Bridge . . . Lock. "Recog. bridge. Kin. image of bridge. Thought that was not the word to be uttered."

Bird . . . Sing. "Kin. of bird. Active suppression. Thought that I must not say it."

III. *Knife . . . Boy.* "Recog. pen-knife. Kin. image of pen-knife. Active suppression by slight locking of the muscles of the throat. Kin. image of boy. Feeling that it was right and said it."

Rake . . . Garden. "Kin. image of rake. Suppressed it by closing the mouth and putting the tongue against the roof of the mouth. Then thought of hoe. Don't know why I did not say it. Then said garden."

Hat . . . Tooth. "Recog. hat. Kin. image of hat. Locked the muscles and said tooth."

IV. *Pear . . . Eat.* "Recog. pear. Thought it was not to be named and said eat."

Car . . . Street-car. "Idea of car. Recalled that I must not say it. Was about to say trolley, when I saw that it had no pole and so said street-car."

Bed . . . Sleep. "Recog. bed. Thought that I must not say the name and the word sleep came."

Cannon . . . Gun. "Idea of cannon. Recalled that I was not to say cannon, so said gun."

V. *Clock . . . Move.* "Recog. clock. Kin. image of clock. Suppressed it by turning to the details of the hands of the clock."

Cards . . . Play. "Kin. image of cards. Suppressed it by turning to details of the cards."

VI. *Car . . . Ride.* "Recog. car. What could I do with it? Ride."

Cannon . . . Fire. "Recog. cannon. Idea of how to use it."

Tree . . . Shade. "Recog. tree. Thought of the appropriate use of it."

Sleigh . . . Ride. "Recog. sleigh and thought of its appropriate use."

VII. *Horse . . . Cow.* "Kin. image of horse. As to the suppression, the word simply did not come. Something providential seemed to prevent it."

Clock . . . Time. "Kin. image of clock. No voluntary suppression."

Collar . . . T–tie. "Kin. image of collar. No voluntary suppression."

Chair . . . Rocker. "Strong tendency to say chair. Inhibition seemed automatic."

Upon examination of the above introspection, we find all forms of suppression from a fully voluntary act to a purely automatic reaction. In all cases the subject begins in the attitude of the negative and positive instructions. As the series progresses, this attitude or 'Einstellung' is less vivid in consciousness. No systematic introspection of the fore-period was required, so that it does not appear in the above. In the first group (I.) there is the recognition of the word, with a tendency to utter it. Then there is the judgment that the word is contrary to the instructions and should not be uttered. A locking of the muscles of the organs of speech follows and the proper association is given. In group II. the judgment is sufficient for the inhibition and the locking of the muscles falls out, while in group III. the muscles are locked without consciousness of the instructions or of a judgment. In group IV. the negative instruction is recalled and is sufficient even to inhibit the kinæsthetic image. In group V. there is a conscious turning to something else, which helps to inhibit the kinæsthetic image. Neither in this group nor in group VI. is there a recall of the instruction or a locking of the muscles. In the latter group a definite attitude, one might say an artificial attitude, is assumed, by which the range of associations is limited to those answering a definite question. This was the attitude of G, which was referred to above. In VII. the process of uttering the word goes as far as the kinæsthetic image, but there is an automatic inhibition. Finally, immediately upon recognition of the picture the association is given. This is the complete automatic suppression. It was not deemed necessary to give examples of this form.

It must not be supposed that the subjects showed this gradual development. The different forms are scattered throughout the different days and the different subjects. From our knowledge of the shortening of the voluntary act into the automatic reflex in other spheres, we can conjecture how it would be in the case of one exercising the power of suppression for the first time, and from the above we can gain an idea of the probable number of stages and the manner in which they present themselves to consciousness.

The reaction times show little of interest as regards the subject of this paper. There is in general a decrease from day to day. In all

but a few cases the R time is shorter than the A time. This decrease is probably due partly to greater practice in the suppression; it is also due to the effect of practice in association and to the fact that in the R-tests the recognition of the picture is more rapid. There should be a coördination between the decrease in the reaction time and the decrease in the amount of kinæsthic imagery. This we do not find. If the only difference between two tests is the absence of the kinæsthetic image in one of them, the test without the image will naturally have the shortest reaction time, but the latter test may have intermediate associations or a seeking for an association, which the other test does not have, in which case its time will be longer.

It is often possible to trace the effect of the inhibition caused by the negative instruction beyond the words actually covered by the instruction. We can thus gain some idea of its sphere of influence. It will be remembered that the subject was told he could name any part of the picture or could describe it. Hardly ever was this done. The associations were, as a rule, away from the picture. The majority of the words were nouns or verbs. Generic names were rare. Adjectives were almost never employed. In fact we have instances where they came to the mind of the subject, but were not used. In the association *Fish* . . . *Water*, the introspection says: "The fish seemed very small and I thought of a very small fish." Again, *Bed* . . . *Child*, "Recognized bed. Thought it a rather small bed. Thought of its purpose. Had a visual image of a child in bed and said child." To the association *Teeth* . . . *Gums*, the subject gave: "Kin. image of false teeth. Should have said these words. I don't know how gums got in." There are several instances in which a word was suppressed because it was either an element of the compound name of the picture or a compound word of which the name of the picture was one of the elements. To the association *Car* . . . *Track* was said: "I almost said car, then trolley. I gave track, feeling the assonance between the tr of trolley and that of track." Two subjects reacted to the picture *Strawberries* with *Fruit*. One of the subjects said: "I recognized strawberries and suppressed the kinæsthetic image of the word. I thought of berries, but suppressed it because it seemed too closely related to the name." The other subject said: "I had a kinæsthetic image of strawberries. I suppressed straw and almost said berries. Then there was a feeling of confusion before fruit came." The introspection to the reaction *Drum* . . . *Beat*, reads: "I recognized drum and the sticks on it. I thought of drum-major, but it did not seem to be the word I wanted." To the reaction *Foot* . . . *Shoe*, the subject gave: "I recognized foot and

thought of saying bare-foot, but it seemed too much like foot and I said shoe." To the reaction *Eye . . . Shoot*, the introspection reads: "I recognized eye and had a kinæsthetic image of bull's-eye, but could not say it." There is a good example of where an idea is suppressed because it occurs with that of the picture. To the reaction *Strawberries . . . Eat*, the subject said: "I recognized strawberries and thought of strawberries and cream, but said eat." There is also an instance where the name of a part of the subject is suppressed. The reaction was *Foot . . . Shoe.* The subject said: "I recognized foot and had a kinæsthetic image of toe, but could not say it." In an A-test the picture clock was named. In the R-test the subject said: "I recognized clock, but could not get anything. It was very queer. Then I remembered that I had named the object and so said clock." In another instance the name of the object, which had been given in the A-test, was entirely suppressed with the exception of its first letter in the R-test. In an R-test the subject gave *Tree . . . Tree* for *Tree . . . Apple.* He said: "I recognized tree and had the idea of an apple, but suppressed it. I don't know why." The reason probably is that the word apple had previously been suppressed in an A-test when it was the name of a picture. As a last example of the force of the suppression may be cited the reaction *Top . . . Boy.* The introspection reads: "Top was coming in when cot was substituted as an auditory-kinæsthetic image. Boy (boycot) succeeded." Here not only was top suppressed, but the similar word cot.

Summary.

It has been clearly shown in the experiment just described, that a negative instruction fulfils the purpose for which it is intended. There are a positive and a negative 'Aufgabe,' both of which are carried out. The negative 'Aufgabe' has acted as a block, cutting out a definite association. In all positive attitudes, one can say that there is also a negative quality, in as much as the way is blocked for all associations not included in the instructions. The difference here is that the negative attitude is a conscious one at first and directed toward a definite goal.

We have also seen evidence of the force of the suppression, which not only inhibits the name of the picture, whenever there is a tendency for it to be pronounced, but frequently inhibits words closely related to the picture. This is similar to the inhibition, so frequently mentioned by Freud, of thoughts which are unpleasant to the subject.

Finally, the general development of the suppression process, especially as shown in the decrease of the kinæsthetic image, tends to prove that the suppression can be strengthened by practice.

No. 6

[Reprinted from The Psychological Review, Vol. XVIII., No. 6, Nov., 1911.]

SUPPRESSION WITH NEGATIVE INSTRUCTION[1]

BY H. S. LANGFELD

Tests with Alcohol and Caffeine and on Cases of Dementia Præcox and Manic Depression

In a previous article[2] the nature and development of the act of suppression with negative instruction in association tests under normal conditions are described. The object of this paper is to give the effect of alcohol and caffeine upon the power of association and reproduction as well as upon the act of suppression. Introspective data upon the process of suppression is also included. The results of the association tests with suppression in cases of dementia præcox and manic depression will also be briefly described at the close of the paper. The same conditions as in the previous experiment prevailed for these experiments. Simple pictures representing a single object were shown and the subject told to give the first word suggested by the picture. As two of the subjects figured in both series new pictures, but of the same nature as the others, were used, except in the case of the tests on the insane, where the old pictures were employed. In all 120 pictures were used, 10 of which were shown on each test day. With few exceptions a test was made every week. The previous instructions were altered so that the subjects were told not only not to name the picture, but not to name any part of it. This was found necessary because one of the subjects got 'set' to name a part of the picture. Except in the case of the insane subjects full introspection on the main period was required at all times. After the experiment had been some weeks in progress Professor Titchener's suggestion[3] in regard to introspection upon the fore-period was followed. The reaction time was taken with a

[1] From the Harvard Psychological Laboratory.

[2] 'Suppression with Negative Instruction,' *Psychological Bulletin*, June, 1910, Vol. VII., pp. 200–208.

[3] E. B. Titchener, 'A Text-Book of Psychology,' p. 460.

Table I

Tests with Alcohol

Subject A

Al = alcohol test, N = normal test, A = association test, R = reproduction test, R.T. = reaction time, k′ = any kinæsthetic imagery, k″ = kinæsthetic imagery of the name of the picture, v = visual imagery, a = auditory imagery, S.S. = successful suppression, S.R. = successful repetition.

	No.	A		R		A				R				A	R
		R.T.	m.v.	R.T.	m.v.	k′	k″	v	a	k′	k″	v	a	S.S.	S.R.
N	1	1.7	.30	1.7	.16	4	2	6		2	1	2		10	8
Al	2	1.6	.36	1.3	.16	5	2	4				1		9	8
N[1]	3	1.2	.09	1.0	.13	3	1	6		4		6		9	7
Al	4	1.1	.15	1.1	.10	10	4	5		6	3	7		8	10
N	5	1.7	.58	1.3	.16	6	3	6		4	1	3		8	9
Al	6	1.5	.24	1.1	.10	6	2	11				7		10	9
N	7	1.6	.16	1.5	.20	5	1	14		3	2	6		10	9
Al	8	1.3	.16	1.2	.10	9	4	5		1	1	1		9	8
N	9	1.6	.14	1.3	.28	15	3	8	1	3		2		10	6
Al	10	1.4	.46	1.1	.16	4	1	2		1	1	1		10	9
N	11	1.5	.21	1.3	.18	9	4	2		3	1			10	9
Al	12	1.3	.27	1.0	.20	5	3	1		4	3			9	10
N	av.	1.5	.24	1.3	.19	Total 42	14	42		19	5	19		% of un- 3%	18%
Al	av.	1.4	.27	1.1	.14	Total 39	16	28		12	8	17		successful 8%	10%
														tests	

[1] Contains only nine reactions.

Subject B

	No.	A		R		A				R				A	R
		R.T.	m.v.	R.T.	m.v.	k′	k″	v	a	k′	k″	v	a	S.S.	S.R.
N	1	1.8	.40	2.4	1.30	15	10	1		7	4			10	8
Al	2	1.7	.26	1.6	.40	16	11	3		15	8	1		7	8
N	3	1.6	.11	2.0	.19	10	6	2		10	7	1	1	9	7
Al	4	1.6	.15	1.5	.37	16	11	1	1	6	5			9	6
N	5	2.0	.72	1.8	.33	10	7	2	1	9	6			8	6
Al	6	1.4	.18	1.1	.22	4	4		4	3	3	2		10	9
N	7	1.7	.36	1.5	.46	13	11	3		6	3			9	8
Al	8	1.9	.44	1.4	.28	12	10	2		7	7			10	8
N	9	1.7	.21	1.5	.26	11	9		1	4	2			8	9
Al	10	1.6	.28	1.2	.22	13	8			5	5			7	8
N	11	1.7	.30	1.4	.29	6	3	2		4	2			10	9
Al	12	1.5	.30	1.1	.10	7	4			7	6			6	9
N	av.	1.7	.35	1.8	.47	Total 65	46	10	2	40	24	1	1	% of un- 7%	20%
Al	av.	1.6	.27	1.3	.26	Total 68	48	6	5	43	34	4		successful 14%	17%
														tests	

stop watch. There were six normal subjects, two figured in the alcohol tests, two in the caffeine tests and two in the tests under normal conditions. These last tests were introduced as a check in order to be sure that the results obtained in the

Table II

Tests with Caffeine

Subject C

Ca = caffeine tests, N = normal tests

	No.	A R.T.	A m.v.	R R.T.	R m.v.	A k′	A k″	A v	A a	R k′	R k″	R v	R a	A S.S.	R S.R.
N	1	4.1[2]	1.1	2.4[2]	.80	8	7	6				2		10	9
Ca	2	2.2	.40	1.5	.10	2	2	7		4	4	5		9	10
N[1]	3	2.2	.40	1.5	.19	4	3	4		2	2	2		9	8
Ca	4	1.8	.33	1.4	.27	4	3	2		4	4	2		8	7
N	5	2.2	.68	1.7	.58	6	6	1	1	3	3			8	8
Ca[1]	6	1.9	.46	1.3	.28	5	5	4		4	4			9	9
N	7	2.2	60	1.4	.18	8	6	5		7	7			9	9
Ca	8	2.0	.34	1.5	.10	5	5	4		4	4			9	9
N	9	1.9	.34	1.3	.14	8	8	3		6	4			9	9
Ca	10	2.2	.56	1.3	.16	4	4	8		4	2	1		10	8
N	11	2.3	.58	1.3	.28	6	4	3						9	8
Ca	12	1.7	.21	1.4	.32	6	6	4		5	3			8	10
N	av.	2.2	.52	1.4	.27	Total 40	34	22	1	18	16	4		% of unsuccessful tests 8%	12%
Ca	av.	2.0	.38	1.4	.21	Total 26	25	29		25	21	8		10%	10%

[1] Contains only nine reactions.

[2] Unusually long R.T. due to inexperience and which it was deemed better to omit.

Subject D

	No.	A R.T.	A m.v.	R R.T.	R m.v.	A k′	A k″	A v	A a	R k′	R k″	R v	R a	A S.S.	R S.R.
N	1	1.8	.34	1.4	.27	1	1	2	6	1		1	2	10	8
Ca	2	1.5	.14	1.2	.08	12	9	2		3	3		2	9	9
N	3	2.0	.37	1.5	.21	12	9	2		2	1	1		10	9
Ca	4	1.6[1]	.22	1.2	.11	8	7	2		2	2	2		7	9
N	5	1.8	.28	1.7	.44	8	7	7		4	2	2		10	9
Ca	6	2.0	.30	1.7	.45	13	11	4		8	6	1		10	7
N	7	2.3	.50	1.5	.30	15	8	1		2				10	9
Ca	8	2.1	.26	2.2	.50	15	9	6		14	9	1		10	8
N	9	1.9	.27	1.7[1]	.39	13	9			12	11			10	7
Ca	10	1.9	.39	1.8	.50	9	9	3		3	3			9	9
N	11	2.3	.41	1.7	.35	11	8	1		4	4			10	9
Ca	12	1.9	.32	1.7	.32	18	7	2		10	8	1		10	8
N	av.	2.0	.36	1.6	.33	Total 60	42	13	6	25	18	4	2	% of unsuccessful tests 0%	12%
Ca	av.	1.8	.27	1.6	.33	Total 75	52	21		40	31	5	2	7%	17%

[1] Contains only nine reactions.

alcohol and caffeine tests were due to the drugs and not to possible differences between the character of the pictures used on drug days and those used on normal days.

Thirty c.cm. of 95 per cent. alcohol in 60 c.cm. of water

were given on alternate test days to the alcohol subjects (Al-subjects). It was taken three quarters of an hour before the test began. This amount of time was required for the alcohol to

Table III

Tests under Normal Conditions

Subject E

Dg = those days on which the same pictures were used that were used for the foregoing subjects on drug days. The averages of these days are taken just as if they were drug days.

	No.	A		R		A				R				A	R
		R.T.	m.v	R.T.	m.v.	k′	k″	v	a	k′	k″	v	a	S.S.	S.R.
N	1	1.5	.26	1.3	.21									7	6
Dg[1]	2														
N	3	1.3	.15	1.2	.24									8	5
Dg[1]	4														
N	5	1.0	.10	1.2	.40	2	2		1	2	2		2	6	6
Dg	6	1.3	.34	1.9	.12	3	3	2		2	2	2		8	9
N	7	1.2	.19	1.1	.26	4	3	1	3	2	2	1		7	8
Dg	8	1.4	.20	1.1	.16	3	2	4		2	1			8	9
N	9	1.5	.36	1.0	.06	6	6		1	2	2			9	7
Dg	10	1.7	.30	1.1	.10	2	2	1	1	1	0			9	9
N	11	1.3	.22	1.1	.16	4	3	3	2	3	2			9	8
Dg	12	1.1	.12	1.0	.10	5	5		1	4	4			8	8
N	av.	1.3	.21	1.1	.22	Total 16	14	4	7	9	8	1	2	% of un- 23%	32%
Dg	av.	1.3	.24	1.2	.12	Total 13	12	7	2	9	7	2		successful 17%	12%
														tests	

[1] It was impossible to use the data of these days on account of an unforeseen irregularity in the experiment. They would, however, have made no difference in the av. R.T.

Subject F

	No.	A		R		A				R				A	R
		R.T.	m.v.	R.T.	m.v.	k′	k″	v	a	k′	k″	v	a	S.S.	S.R.
N	1	2.7	.88	1.9	.26			3				1		10	6
Dg	2	2.3	.65	1.3	.30	2	2	5	2	5	4	1		8	9
N	3	1.8	.42	1.2	.20	1	1	3	2			3		10	8
Dg	4	1.9	.39	1.5	.23	4	3	5	0	1	1	4	1	9	7
N	5	1.7	.22	1.7	.58	1	1	6		2	1	2		10	6
Dg	6	2.1	.52	1.3	.18	9	4	6				2		10	6
N	7	1.8	.44	1.3	.24	1	1	5		1	1			9	10
Dg	8	1.9	.40	1.6	.30	5	4	7		1	1	3		10	9
N	9	2.3	.48	1.3	.16	3	1	8				1		9	8
Dg	10	1.6	.34	1.4	.10	4	1	7		3	2			10	9
N	11	2.0	.40	1.3	.28	2	2	8						10	7
Dg	12	1.8	.26	1.4	.28			2				1		9	7
N	av.	2.0	.47	1.4	.29	Total 8	6	33	2	3	2	7		% of un- 2%	25%
Dg	av.	1.9	.43	1.4	.23	Total 24	14	32	2	10	8	10	1	successful 2%	17%
														tests	

show sufficient effect. The tests took place in the morning some hours after eating. Six gr. of caffeine in capsule were given on alternate days to the caffeine subjects (C-subjects). Three gr. were taken one and a half hours and three gr. one half hour before the test. On the other test days capsules of sugar of milk were given in order, as far as possible, to avoid suggestion. A like precaution would have been taken with alcohol if a suitable disguise for the alcohol could have been discovered.

Let us first examine the reaction time for the alcohol tests (Al-tests). The average reaction time for association for all the Al-days shows a decrease of 1/10 sec. as compared with the average time for all the normal days (N-days) in the case of both Al-subjects. With this decrease in reaction time, however, there is less success at suppression. One subject has 14 per cent. failures with alcohol to 7 per cent. without. The other 8 per cent. with to 3 per cent. without.

The C-subjects show a greater decrease in the reaction time for association on C-days. The average for the C-days is 2/10 sec. more rapid than for the N-days. As to the quality of the reaction one subject shows an increase of 2 per cent. in failures, the other an increase of 7 per cent.

In the Al-tests the R.T. for reproduction on the Al-days shows for one subject a decrease of 2/10 sec. and for the other a decrease of 5/10 sec. and in both cases there are slightly less failures to reproduce correctly on the Al-day. There is, then, a decided increase in rapidity to reproduce the former association with, if anything, more successful reproduction on the Al-days.

Caffeine, on the other hand, has no effect on the R.T. for reproduction of either subject. The one subject has 2 per cent. less failures, the other 5 per cent. more.

The N-subjects never show a difference of more than 1/10 sec. between the two sets of pictures either in the R.T for association or for reproduction. In the association test there is a decrease of 1/10 sec. for one and the same R.T. for both sets of pictures for the other. In the reproduction one shows the same R.T., the other an increase of 1/10 sec.

As to the failure to suppress, one N-subject shows no dif-

ference, the other one of 5 per cent., between the two sets of pictures. In the reproduction one shows a difference of 8 per cent., the other one of 20 per cent.

The slight differences in the R.T. of the N-subjects seem to show that the noticeable differences in the R.T. for the other subjects are not due to differences in the material used. The differences in the failures between the two sets of pictures are sufficiently great to prevent any other deduction in the case of the other subjects, than that neither alcohol nor caffeine markedly affect the number of failures in suppression or reproduction. This is well to know in the case of the decrease in reproduction time by alcohol and in the association time by caffeine. The tests have been made on too few subjects for any final assertion to be possible, but the results at least suggest that as much as 30 c.cm. of alcohol do not affect to any appreciable degree a control of one's thoughts or speech such as is necessary in the suppression here required, while that amount of alcohol does increase the rapidity without impairing the accuracy of the reproduction of associations recently made. Caffeine has a quickening effect upon thought. The associations are made more rapidly and the power of suppression is not seriously impaired. This is the effect that casual introspection generally attributes to the drug. It has no decided effect on the reproduction.[1]

[1] Aschaffenburg writes in regard to the effect of alcohol: "Eine qualitative Veränderung der Arbeitsleistung wurde durch den alcohol nicht hervorgerufen." ('Praktische Arbeit unter Alkoholwirking,' *Psychologische Arbeiten*, Bd. I., p. 626.) Ach's observation that alcohol causes an "Herabsetzung der Schnelligkeit und der Verkleinerung des Blickfeldes der Wahrnehmung" ('Ueber die Beeinflussung der Auffasungsfähigkeit durch einige Arzneimittel,' *Psychologische Arbeiten*, Bd. III., p. 288) may in part at least account for the lack of decrease in the R.T. for associations with alcohol, for we know that the motor discharge is aided by alcohol. In the reproduction tests, where the pictures are familiar, the influence of alcohol on the perception is not as great, or not sufficient, at any rate, to offset the increase in the rapidity of the motor discharge. It may be that the decrease in the time for the motor discharge, which, of course, would allow the word to be spoken more quickly, is the sole cause of the decrease in the R.T. for reproduction. For the influence of alcohol on the motor discharge see Ernst Rodem: 'Ueber die Dauer der psychischen Alkoholwirkung,' *Psychologische Arbeiten*, Bd. IV., pp. 40–41. Ach says further that "Caffein bewirkt eine geringe Besserung der Auffasung" (ibid.). This may be a cause of the decrease in the R.T. for associations with this drug. From August Koch's and Emil Kraepelin's observation that "Der Ablauf gewohnheitsmässiger Associationen wird durch das

A record was kept of the different types of images as is shown in the tables. It was thought that the drugs might change the type and it was also considered of interest to determine if the different reaction times could be correlated with the different types. The results seem negative. There is in general little difference between the imagery in normal days and drug days and nothing can be deduced from the differences that do occur since the normal subjects show differences as great. As to the correlation of types of imagery with reaction time for association, there is a tendency for those who have much visual imagery to have longer reaction times. This may be observed in subjects C and E. Subject A, however, is an exception. Subject E, who has the least amount of visual imagery, has the quickest R.T.

An examination of the quality of the reaction words has also been made to see if there is a change in the distribution under alcohol and caffeine. It was thought that although there was slight effect upon the actual suppression of the name of the object, yet the quality of the reaction words might perhaps give a clew among other things, to the difficulty of suppression with and without the drugs. A classification was chosen which was expected to bring out any such change in distribution. An increase in the number of descriptive words or of those words caused by suggestion through resemblance might indicate a difficulty to get one's thought away from the picture. An increase in repetition or a change in the number of super-ordinate words, or in the type of reacting under the influence of a drug, would also be of interest. The decision as to whether a word should be classified under contiguity or coördination is, as is well known, often a difficult one. Without introspection it is always a guess.

Turning to the table we find a surprising similarity in the distribution of the words on drug days and normal days. There is, however, one difference of interest. Both caffeine subjects, *C* and *D*, have fewer words from suggestion on C-day. C has a change from 7 to 2 words and D one from 18 to 4.

Caffein erleichtert" ('Ueber die Wirking der Theebestandtheile auf kōrperliche und geistige Arbeit,' *Psychologische Arbeiten*, Bd. I., p. 488) one would expect that caffeine would quicken the reproduction. This was not the case in our experiments.

Table IV

Classification of Association Words

	Subject A			Subject B			Subject C			Subject D			Subject E			Subject F		
	Al	N	Total	Al	N	Total	Ca	N	Total	Ca	N	Total	Dg	N	Total	Dg	N	Total
Descriptive	2	2	4	8	6	14				1	1	2	3	2	5			
Verb	32	35	67	4	1	5	1	1	2	1		1	16	9	25	43	42	85
Contiguous	14	15	29	25	24	49	41	37	78	18	23	41	3	4	7	1	4	5
Superordinate				1	3	4				3	1	4	1		1			
Coördinate				4	4	8	3	4	7	18	13	31	4	2	6			
Repeated	6	2	8	1	1	3	2	1	3	4	1	5	2	5	7	6	7	13
Suggested		2	2	3	2	5	2	7	9	4	18	22	3	8	11	3	3	6

As will be seen there is a tendency for the subjects to react always with a word of the same class; that is, they become stereotyped in the quality of reaction words. In the previous work[1] we mentioned this tendency of one of the subjects to react with verbs. In the present experiments we find two distinct verb types, A and F, and one fairly marked verb type E, two contiguity types, B and C, and one type, D, in which most of the words are divided between contiguity, coördination and suggestion. It is interesting to note that the subject E, who became the least stereotyped, also has the most failures to suppress. We will return to the discussion of these failures later. Subject B, who has the most descriptive reactions, also has a comparatively large number of failures to suppress, which fact in some part bears out the hypothesis that reaction by descriptive words shows a certain difficulty in getting away from the name of the picture. There is no correlation between these types and the reaction times.

Unfortunately the attitude of the subjects in the fore-period was not ascertained at the beginning of the experiment, so that it is impossible to give an account of the development of the consciousness of the instruction during this period. Later, however, the contents of consciousness during the fore-period were occasionally tapped by interrupting the experiment before the picture was exposed and without warning to the subject. It was found that the negative instruction, which was always repeated by the experimenter at the beginning of each series,

[1] P. 206.

continued to 'ring in the ears' of the subjects, during the fore-period of the first test, or was repeated by the subject in its negative form. As a rule after this first test there was nothing in consciousness pertaining to the instruction. The subjects described this attitude as 'passive.' They sat before the shutter and waited for it to open. This expectancy was all that could be found in consciousness. One subject, E, proved an exception. He repeated the instruction in its negative form before almost every exposure. Sometimes this was reduced to 'No, no,' or 'not picture or any part of it.' Several times it was in the form of 'do what you did before.' This is a positive command, whose purpose is to suggest a negative attitude. Beyond the mere repeating of the words of instruction the only representation in consciousness of the negative attitude was a focusing of the eyes on a point beyond where he knew the picture was to appear. It was in this background that he seemed to himself to search for a word, after recognizing the picture, and he felt that this attitude aided him in getting away from the name of the picture. In almost no instance was the negative instruction changed to a positive one. The words touched off a cortical set, which may be called a negative set, in so far as it has an inhibitory effect either upon the thought of the name of the object or its expression in speech. The nature of this set cannot be described further, but that there are two different processes, a positive and a negative one, seems fairly well established.[1] Although little was found in consciousness beyond the words of instruction to represent this negative set, one may hazard a guess that in the early development of the individual there is a characteristic representation, but that, following the laws of habit the process becomes more or less mechanical and is generally limited to physiological processes.

A full description of the process of suppression during the main period is given in the former paper. These experiments furnish no new data. The introspection does, however, throw some light upon the causes of failure to suppress and failures in reproduction as well as some description of the effect of the

[1] See summary of former paper, p. 208.

inhibitory attitude. As the tables show, there were few failures to suppress. Subject E shows the most. He is also the one subject who repeated the instruction before almost every test. When he failed to suppress he generally said that his attitude was not good. Either he had not repeated the instruction or he hadn't the instruction well in hand. There seems to be strong indication here that the subject lacked concentration. In terms of this problem the instruction not only tended to disappear from consciousness as was the case with the other subjects, but the attitude as represented in physiological processes tended also to disappear, so that the instruction had to be repeated. Like indications of lack of concentration were observed in a different experiment conducted by another experimenter in this laboratory.

When there was difficulty in recognizing the picture, the delayed act of recognition with its affective tone seemed to weaken the attitude of suppression and the picture was frequently named. A possible description is that the name of the object is delayed by lack of recognition, and when this recognition occurs the name comes into consciousness with a bound, so to speak, and as the attitude of the instruction had been pushed aside for the moment by the difficult act of recognition, the name comes to utterance.

An emotional state of one of the N-subjects, who had a momentary difference with the experimenter on one of the test-days, also caused an unusual amount of failure. An occasional unavoidable distraction also weakened the attitude. An attempt was made to conduct similar experiments under conditions of distraction, but it was found that distraction, such as is produced by noise, odors, mental work, or emotional states could not be made subject to the will of the experimenter.

As regards the failure to reproduce the previous association, one of the chief causes is the fact that there were several images in consciousness before the one given in the association test. In the reproduction test that followed, one of these images, and not the one actually given as an association, was then reproduced. For example: association "BRUSH—*hair*. Recognized brush. Kin. image of brush. Active suppression.

Visual of side of horse with brush on it. Then hair." Reproduction '*brush—horse.*' There were also instances of the reproduction of a word that had just preceded instead of the correct word.

Varied effects of the inhibitory process could be observed in the introspection. At times a certain perception was suggested by the shape of the picture. As soon as it was seen not to be the name of the picture, this perception was given as an association, even though the object itself may not have been fully recognized. For example: association "CARPET-SWEEPER—*typewriter.* Started to recognize typewriter. Saw it wasn't. Said typewriter." Association "RATTLE—*hair.* Percept slow in forming. After slight pause hair came. Suggested by picture although I knew it wasn't that." Association "RAT—*dog.* Recognized it wasn't dog. Looked so much like it that I said dog. Then recognized rat." Here is also an example where the name would not come. Associaton "BATH-TUB—*bowl.* Percept came somewhat slowly. Tried to get name but couldn't. Then came word bowl. The form suggested bowl." Even when the association is not by suggestion the name may be held up until after the association, both in association and reproduction tests. Association "BLOCK—*axe.* Axe came immediately. Afterward chopping block," or reproduction "PITCHER—*water.* Water came automatically. Kin. of pitcher afterward."

There are also instances where the inhibitory process instead of inhibiting the name of the object gives it a different meaning, attaching the word to imagery sometimes quite different from the picture. This very frequently occurs with verb associations; for example, association "FLY (noun)—*fly* (verb). Its wings were prominent," or association "BRUSH (noun)—*brush* (verb). Recognized brush. Visual image of brushing." Association "SMALL TENT—*circus tent.* Thought of that tent, then large circus tent." Association "LADDER—*step.* Aud. ladder. Persisted a short time. Made visual image of step to a house. Then said step."

Tests on Insane Subjects

Tests of a nature similar to the above were made upon dementia præcox and manic depression cases at the Danvers State Hospital.[1] A similar instrument and sixty of the cards used in the first series of normal tests were employed. Introspection was not required, inasmuch as there would always be grave questions as to its reliability. The instructions were those of the first normal tests and did not include prohibition to name a part of the picture. There were eight dementia præcox, three manic and one depression patient.

Table V

Tests on Dementia Præcox and Manic Depression

No. = number of tests, F.S. = number of failures to suppress, F.R. = number of failures in reproduction.

Subjects		A Tests			R Tests			F.S.	F.R.	Classification of Association Words						
		No.	R.T.	m.v.	No.	R.T.	m.v.			Des.	v.	Con.	Sup.	Coö.	Re.	Sug.
Dementia Præcox	H	58	3.3	1.56	58	1.5	.33	19	7	1		20	20		5	
	R	51	6.4	3.07	42	4.5	2.14	51	60	6	1	7				
	Ah	58	4.9	2.23	58	1.8	.65	0	7			27	13	3		4
	Cu	47	5.5	2.59	47	1.9	.54	21	9	10	4	10	6		1	
	S	58	3.2	1.56	58	1.9	.66	2	7	4	18	24	3	2		
	M	56	3.7	1.05	56	2.5	.90	0	9		1	43			1	1
	Ch	57	2.1	.76	56	1.1	.25	9	5	2	3	34	2	2		
	T	55	2.8	.92	55	1.3	.23	11	14	7	5	14	7	2		
Depression	N	58	4.5	2.03	58	1.9	.65	5	12	2	16	15	15			
Manic	E	55	2.3	.75	55	1.5	.35	5	5	2	1	34	11		4	
	Al	40	3.4	1.29	40	2.9	2.20	0	7	31	1	6				
	G	39	1.2	.38	39	.75	.13	3	0	3	29	6				

From the table we see that the reaction times for association are in all but two cases longer than for any of the normal subjects—in many instances longer than could be accounted for by a difference in education. These long R.T. occur both for dementia præcox and manic depression. Among the manic depression the manic type is more nearly normal. One showed an unusually rapid reaction time. As regards the R.T. for reproduction in all but three cases it closely resembles that of the normal subjects. Many of the m.v. are extremely large.

As regards suppression, the three cases which show many

[1] The author takes this occasion to thank the physicians of the hospital for their kindness in permitting and facilitating these tests.

failures are all dementia præcox. One has a very great many failures, the others have more than the normal subjects with the exception of normal subject E, who had most of his failures during the emotional disturbance. The one patient with the largest number of failures also has the largest number of failures to reproduce the correct word.

The fact that all the large number of failures to suppress are by dementia præcox patients seems to corroborate the theory that an impairment of the will in which a decrease in attention plays an important part, is a characteristic feature of this disease.[1] The lack of retentiveness in the case of one dementia præcox patient seemed to be a special feature of the case not found in any of the others. In her case the symptoms of dementia were much more marked.[2] A dementia præcox patient not recorded named the object at every exposure. He showed a willingness to take part in the experiment and whether his failure to suppress was due to any difficulty upon his part to restrain from uttering the name or from a negative attitude toward the experiment could not be decided.

These subjects also show distinct types of reaction as regards the quality of the reaction word. Most were of the contiguity type, one was distinctly of a verb type, two showed many superordinate words and one was decidedly of a descriptive word type.

Summary

As was mentioned above, there were too few subjects to permit of any generalization. Several interesting possibilities, however, were suggested by the results.

1. 30 c.cm. of alcohol caused a decrease in the reaction time in the reproduction tests. It did not appreciably affect the suppression or accuracy of reproduction.

[1] Alfred Busch, 'Auffassungs- und Merkfaligkeit bei Dementia Praecox,' *Psychologische Arbeiten*, Bd. V., p. 336.

[2] Dr. Charles Ricksher remarks in regard to the retentiveness of dementia præcox patients, " . . . when cases are arbitrarily classed according to the apathy they show, the duration of the disease, etc., those showing the more marked deterioration almost invariably show less ability to recall either the auditory or visual stimuli than do those with a slighter degree of dementia." ('Impressibility in Dementia Præcox,' *American Journal of Insanity*, Vol. LXVI., No. 2, p. 229.)

2. Caffeine caused a decrease in the reaction time for association and showed no appreciable effect upon the suppression or accuracy of reproduction.

3. Introspection on the fore-period showed no evidence of the necessity of translating negative into positive instruction. This makes it probable that there is a distinct negative as well as positive attitude, which in most instances can be described solely in terms of cortical set.

4. The lack of a power of suppression was found only in some of the dementia præcox patients. The manic depression patients were normal in this respect. Accuracy of reproduction was normal in both dementia præcox and manic depression with one exception (dementia præcox).

No. 7

Vol. XIII
No. 3

PSYCHOLOGICAL REVIEW PUBLICATIONS

MAY, 1911
Whole No. 55

THE

Psychological Monographs

Some Types of Attention

BY

H. C. McComas, Jr., Ph.D. (Harvard)

Princeton University

An Investigation Conducted in the Harvard and Princeton Psychological Laboratories

THE REVIEW PUBLISHING COMPANY
41 NORTH QUEEN ST., LANCASTER, PA.
AND BALTIMORE, MD.

COMPOSED AND PRINTED AT THE
WAVERLY PRESS
BY THE WILLIAMS & WILKINS COMPANY
BALTIMORE, U. S. A.

CONTENTS

Part I. Introductory

Part II. Experiments

Part III. Correlations

Part I. Introduction

THE PLAN AND SCOPE OF THE EXPERIMENTS

The experiments described in the following pages were designed to discover whether the differences which appear among individuals, in certain simple acts of the attention, are indicative of typical traits of the attention, or whether they are fortuitous and unrelated. Individual differences in attention are easily found and have been frequently described, but few efforts have been made to determine what traits go together. Popular judgments on such questions are very common. The unconscious sorting out of the bright and the dull, the absent-minded and the alert and many other contrasts, which everyone is constantly making in his daily relations with other people, is a recognition of differences in attention and possible groupings of them for practical purposes. Teachers are very prone to generalize in the estimates of their students, and undoubtedly many of their shrewd discriminations would be borne out by laboratory tests. Though it is exceedingly probable that a great many convictions which have grown up in the classrooms are not based on a sufficient or accurate induction. Thus, the common 'rules' that a fast reader remembers less than a slow one, or that the student who learns quickly forgets more easily than the plodder are specimens of the popular correlating which would quickly lose their force under careful observation or experiment.

In endeavoring to detect typical traits of the attention the series of experiments to be described were planned to consider several characteristic attentional acts and to ascertain the individual differences which appeared in each. These differences were then studied and correlations sought by a mathematical formula. Throughout the entire work each

experiment has been reduced to as simple terms as possible, and every effort was made to make the measurements accurate.

Of course, in all such work the larger the number of individuals studied the better. It is not practicable, however, in a psychological laboratory to examine great numbers and it is impossible in work of the kind in hand to go outside for material, as the apparatus is not easily transported. In all about twenty-five people participated in the experiments. From these it should be evident whether some traits of attention are indicative of types or not; though a larger number must be examined before any far reaching conclusions are attempted. This further study is the more desirable as the subjects were university students and had had from four to seven years of college work behind them; which, of course, would tend to give them similar habits of mind. Far greater diversities in concentration, for example, would probably be found among an equal number of men chosen at random in the street. This uniformity of training is a disadvantage in studies of natural differences, but the advantage of having college-bred subjects in some measure compensates for it. For much of the work calls for introspection, which could hardly be obtained from those who had no such training.

The number, and therefore, the character of the experiments were limited by the conditions of the experimentation. For example; no work on the influence of fatigue upon attention was attempted, when it became apparent that the one hour allotted each experiment would not suffice to induce fatigue in all the subjects; nor was it practicable to find how the personal interests and habits of thought of the various observers affected attention. Questions of involuntary attention and of fluctuations were not studied; not because they are not of importance in finding Types, but because the difficulties to be overcome in preparing conditions were too time-consuming to admit them. To obtain material which would permit accurate comparison of one subject's results with those of each of the others it was thought advisable to limit the field to tachistoscopic work as far as possible. Differences which creep in when the apparatus is frequently

changed, were thus obviated. This restricted the field to visual work, in large measure, but allowed ample scope for individual differences to show themselves. In the tests for 'span' different classes of objects were exposed, which called for different attitudes of attention in the subjects and showed individual differences clearly. So in the other tachistoscopic tests; a comparatively wide variety of experiments was found practical.

The scope of the work, therefore, is limited to groups of university students, each group studied during one college year in one hour periods. The apparatus largely restricts the work to attention in visual perception.

The plan of the experiments under such conditions shaped itself into a study, first, of the characteristic span of attention of each subject in observing several classes of objects, then, the ability to concentrate upon certain things and to inhibit others, this led into an examination of the differences in the faculty of turning the attention from one class of interests to another and the quickness with which this could be done. Throughout the work the experimenter noticed differences in the records of subjects due to the memory factor. These were studied with a view to detecting a possible correlation between memory and attention. Finally, the question of the relation of memory types to types of attention concludes the experimentation. The results are given in each chapter in tables which show the relative capacities of the subjects for that experiment. This determines the subject's rankings which are used in the correlation tables. From the correlations the conclusions are drawn concerning the possibilities of Types of Attention, in the experiments used.

HISTORICAL SETTING

There is very little literature upon Types of Attention. Scattered references may be found in recent studies, but no thoroughgoing effort has been made to find actual typical characteristics in the individual differences in attention. The nearest approach to the experiments in hand are the

tests made for individual differences in several faculties. Some of these we shall consider later.

While the great body of psychological literature is silent upon the matter of Types of Attention it nevertheless bears witness to them in a very unique way. For many writers upon philosophical and psychological, or near-psychological, topics since Plato's time have touched upon the activities of attention, and their opinions give an interesting insight into their own mental processes. We find throughout the history of the theories of attention two general classes of opinion[1]. The one holds that the attention is more or less subjected to the play of stimuli upon the mind, that the inward direction of one's course of thought is to be found in the influence of his surroundings. The other view emphasizes the influence of the mind upon the activity of attention regardless of the external world. It magnifies the domination of a will or a self. Of course the general philosophy of a thinker will modify his conception of each chapter in psychology; but, in spite of that, the disposition of his own thinking will tend to be the criterion by which he will be guided. If he is easily distracted by his surroundings, if he finds it difficult to concentrate upon intellectual problems rather than perceptual, if his attention is of a discursive nature, passing easily from one subject to another, then nothing could be more natural than the conlcusion that the attention is directed by stimuli rather than by will. On the other hand, a mind which readily concentrates upon abstract themes, ignoring the passing appeals of the outer world, feeling its ability to turn now to one phase of the subject considered, now to another, without let or hindrance, is very apt to be a mind which concludes it possesses an independent control over the attention. From such experiences the two divergent types of theory, the involuntary and the voluntary, undoubtedly evolved. They are descriptive of two types of attention, insofar as they are based on intro-

[1]D. Braunschweiger, *Die Lehre von der Aufmerksamkeit in der Psychologie des 18. Jahrhunderts;* L. L. Uhl, *Attention, an Historical Summary of the Discussions Concerning the Subject.*

spection, and few men are so abstract in their treatment of such matters that the evidence of their own mental life does not affect their systems of thought.

Such differences as those appearing between the positions taken by Condillac, Bonnet and Helvetius on the one hand and by Reid and Stewart on the other bespeak more than philosophic and literary differences. The natural differences of race and mental traits come into the writings and account in large measure for their attitudes on the question of attention.

It is perfectly legitimate to assume that we have the materials for two types of attention, at least, in the mental life of psychologists and philosophers. It would be a valuable study if the influence of these fundamental mental traits were traced out in the systems of philosophy which these minds found the most acceptable and congenial.

Baldwin finds five classes of modern psychologists; those who maintain that the affective element is dominant in attention, namely Horwicz and Ribot; those who find in attention an original activity of the mind; Stumpf, Ladd, Ward and Jodl; those who refer the fundamental factors of attention to intensity of sensation and perception and their psychic reinforcement, such as Bradley and Müller; those who see attention as the outcome of inhibition, Ferrier and Obersteiner; and the champions of the theory that attention is to be understood in the light of the motor factors, Baldwin, Lange, and Stout. In the last group of theories we should consider Münsterberg's 'Action theory' and the theories of McDougall and Sherrington.

Not until the time of Francis Galton do we find any consistent study of individual differences.[1] His suggestions were carried out experimentally among school children by Gilbert, Boas, Bolton and others. However, the most nearly related to Types of Attention is the work of H. Griffing *On the Development of Visual Perception and Attention* in which he tries to establish a correlation between the Span of Attention and

[1] *Inquiries into the Human Faculty.* 1883.

the mental ability of the subjects. Baldwin's doctrine that the attention is as subject to ideational differences as memory and imagination, which appeared in his *Mental Development* was not based on any experimental fact. In the work of Henri et Binet, *La Psychologie Individuelle*, 1896, we have the recognition of the fact that attention may differ in typical ways among individuals. Later Binet endeavored to disprove Gilbert's correlation between mental ability and span. The work was done on *auditory* span. Binet and Vaschide made many experiments for differences in reaction-time, memory and general mental ability, but none bearing directly on our subject. The article of S. E. Sharp upon Individual Psychology gives a very good résumé of what work Binet did on attention.[1] He sought to determine the degree of attention by having his subjects cancel certain letters as they read a page. The range of attention was sought by having the subjects read aloud and write the alphabet simultaneously. Both methods are, of course, very faulty and no one should expect clear-cut results from them. In L. W. Stern's *Üeber Psychologie der individuellen Differenzen*, 1900, we have a very able treatment of Types in psychology. In attention he finds the concentration (Konzentration) and the expansive (Elastizität) features of attention indicative of two types. Also, he finds typical differences in constancy of application and the intermittency of concentration. He maintains that the psychic energy used in attention varies. This explains some of the phenomena of differences in morning and evening workers and of sleep as well. He does not attempt any large correlations between types.

Kraepelin and Cron in their work *Ueber die Messung der Auffassungsfähigkeit* made tests of the simpler mental factors and found the results far from satisfactory. This failure of correlation is rather characteristic of experiments in individual differences; witness the paucity of results from the experimentation of this character by Wissler, Thorndike and others upon students. Spearman, nevertheless, believed

[1] *Amer. J. of Psychol.*, x, p. 329.

from his experiments that a correlation could be found between all forms of sensory discrimination and the more complicated intellectual activities of practical life. He sums up his results thus: "All branches of intellectual activities have in common one fundamental function (or group of functions), whereas the remaining or specific elements of the activity seem in every case to be wholly different from that of all others."

The discussions of the work of Münsterberg, Cattell, Jastrow, Calkins, and others which deal with the larger problems of Individual Differences need not engage us here. Our retrospect upon the historical feature of Types of Attention shows the need of a beginning, at least, in the literature of this subject.

APPARATUS AND SUBJECTS

The apparatus used in nearly all of the experiments was a very simple form of tachistoscope. It was fastened to a low table and leaned toward the subject, so that he could conveniently place his eye close to the shutter-opening. It was simply constructed with a base-board 36 x 14 inches which sustained supports holding a similar board above it. This upper board was arranged with an incline toward the subject. It was 16 inches above the base-board along the line farthest from the subject and 12 inches along the line next him. A frame about 3 inches high held an automatic camera shutter over an orifice in the center of this inclined upper board. Immediately beneath was a heavy grey card, parallel with this upper board, and 17 inches from the subject's eye. Upon this card the smaller cards, containing the objects to be observed, were placed. The shutter was controlled by a pneumatic device. A pendulum, regulated to traverse its amplitude twice in a second, hung beside the experimenter's chair. The experimenter was hidden from the subject by a cloth screen which was suspended by a rack on the upper board. The whole frame was covered with black card-board, except the back which was left open for light and to enable the experimenter to change the exposed objects easily.

Nothing was visible to the subject but the screen upper, board and shutter. When the shutter sprang open the white card, 7 x $5\frac{1}{2}$ inches, which held the objects to be observed, was clearly in view. The letters used were $\frac{5}{16}$ inches high and were black. The colored letters were about $\frac{1}{2}$ inch in height, and the colored figures were from $\frac{1}{2}$ to 1 inch in height. Where the squares of color were used, they were about $\frac{3}{4}$ inch each way.

Another apparatus was used in the experiments upon visual and auditory attention which might be termed a rotary tachistoscope. It was a wooden disk 24 inches in diameter hung upon an axle, and connected by a rubber belt with a dynamo whose wheel connections admitted of several speeds. Thirty-eight words in white letters were fastened at regular intervals near the circumference. Immediately in front of the wooden disc was a black card-board disc with slits opposite the words. When these two rotated together, words flashed up before the eye and disappeared without their motion being observable. The eye was kept in position by looking through another slit card-board about 1 foot from the black card disc.

In the work at the Laboratory in Princeton a tachistoscope was constructed from two black card-board discs 50 c.m. in diameter. Each disc had a quadrant cut out of it, and they were rotated with half of the surface of one disc covering half of the surface of the other. Where the cut portions coincided the observer had a clear view of the card to be exposed. Attached to the wheel, on which one of the discs was fastened, were several copper fingers. These made and broke electric currents to magnets and bells, at the moment when the card was exposed.

The subjects participating in the first year's series of experiments were four graduate students in Harvard University, a practicing physician and a professor in psychology. In the second year three of the subjects were instructors, the others being graduate students who had had considerable laboratory experience. Two of these were women. In the third year the nine young men were all undergraduates in Princeton.

PART II. EXPERIMENTS

THE SPAN OF ATTENTION

In measuring attention we feel the need of an exact definition of what we are measuring more keenly than in any other treatment of the subject. Our work will be confusion worse confounded if we do not have a clear-cut idea of just what is being measured. Unfortunately we encounter the old difficulties, which are met in every attempt to define consciousness, those of implying the thing defined in the definition. This 'circulus in definiendo' is a very subtle snare just here. Attention is a characteristic of every mental act, we are always in the center of it, we cannot get away from it and survey its borders from without. Nevertheless, we may make clear what is meant in the present chapter by attention, with illustration and comparison.

When we look at a printed page there is always some portion of it, perhaps a word, which we see more clearly than the rest; and out beyond the margin of the page we are still conscious of objects which we see only in an imperfect way. The field of consciousness is apparently like this visual field. There is always a central point of which we are momentarily more vividly conscious than anything else. Fading gradually away from this point into vague and vaguer consciousness, is a margin of ideas, or objects, of which we are aware in some sort of mental indirect vision. This fact that consciousness always has a focal point, which reveals the momentary activity of the mind, is what is meant by the fact of attention.[1]

This is expressed graphically by means of five concentric circles. The outermost circle representing unconsciousness; the next inner circle, subconsciousness; the next, diffused consciousness; the circle next to the center is called active consciousness or attention and the center circle represents apperception, which synthesizes all mental data.

Our problem is the measuring of this focal area in consciousness. That it is not anything like so simple and static

[1]Angell, J. R., *Psychology*, p. 65.

a thing as the series of circles might imply goes without saying. Indeed, the mobility and elasticity of that central area are equalled only by the vagueness and uncertainty of the several grades or degrees of consciousness represented. It would seem at first sight that the problem of determining where this central clearness ends and the peripheral obscurity begins is as thankless a task as finding the line between candle-light and the darkness enveloping it. There are, fortunately, some features in the process which lend us material aid. As Wundt showed, many years ago, every process of attention has two factors, the one increases the clearness of an idea or perception, the other diminishes, by inhibition, other available impressions or memory-images. So our area of light is heightened in its clearness and our circumference of obscurity is darkened, making the margin of uncertainty between the two narrower than it first appeared, though all too broad at that. Titchener adds,

> Attention, in other words, means a redistribution of clearness in consciousness, the rise of some elements and the fall of others, with an accompanying total feeling of a characteristic kind."[1]

A large number of careful experiments have been made to measure this span of clearness. Usually they have sought to catch the act of attention in an instant of time and to take a sort of psychological snap-shop of its processes. This, of course, applies to attention concerned with spacial perceptions, not with temporal, and it is with the spacial aspect we shall deal. A tachistoscope is usually employed which either illuminates the field observed for a fraction of a second or exposes the field by the opening of a shutter. The observer has a fixation point in the middle of the field which obviates loss of time by eye movement, and makes measurements more exact. Wundt says:

> careful introspection easily succeeds in fixating the state of consciousness at the moment the impression arrives, and in distinguishing this from the subsequent acts of memory . . . these experiments . . .

[1]Titchener, E. B., *The Psychology of Feeling and Attention*, p. 183.

show that the scope of attention, when it is kept at its maximum intensity, remains constant only when the impressions are held apart as in the case of isolated lines, numbers, letters.[1] Six such simple impressions can, under favorable conditions, be apperceived in the same instant. As soon as the impressions are bound together in complexes the number included in the scope of attention changes.

This conclusion is borne out by the experiments of Goldscheider and Müller at Berlin[2]. They found that for isolated lines exposed for only $\frac{1}{100}$ of a second, four or at most five could be recognized. When the lines were grouped in symmetrical forms a larger number could be apperceived. Cattell, Erdmann and Dodge corroborate these findings in their experiments upon the psychology of reading.

We may be sure, then, that the process of attention when concerned with a few unrelated impressions is unlike the process with which we are familiar in the ordinary activity of attention; where we naturally associate and assimilate the impressions and retain these synthesized groups in our memory. But we may not be sure as to just how far these so-called unrelated objects fail to be apperceived. In a few trial experiments upon this problem I quickly discovered that the ability to apperceive in groups varied in the same individual for different arrangements and varied widely in different individuals. Not only was this confusing situation present, but a more subtle difficulty presented itself in the fact that a certain amount of grouping may occur in an unconscious manner. That is, the lines vaguely suggest a box, a house, or a face, but not clearly enough to make it a matter of comment, or even of notice, unless questioned by the experimenter. In such cases the span of attention will appear large, though the cause is a purely accidental one. This occasional activity, the assimilating of impressions into larger wholes, cannot be entirely excluded from any experiment upon attention. It is one of the several disconcerting factors which we must seek to minimize. I found, however,

[1] Wundt, W., *Outlines of Psychology*, Third English edition, p. 236.
[2] *Zeitschrift Klin. Med.*, Bd. xxiii, p. 131.

in many hundreds of experiments that the subject's introspection and the report he gave went very well hand in hand. That is, when a series of words suggested a sentence and so gave unusually large returns, or when several colors grouped themselves as shades of a fundamental color, I could detect the ease in which the report was made and so corrections could be made, or the result discarded. I do not believe that this grouping is as prevalent in those experiments where the impressions themselves are a complex such as words, colored figures, or geometrical forms, as in those simpler presentations where it is difficult to arrange mere dots and dashes so that they will not suggest some form or figure to the eye. Rather the association will arise from alliteration or rhymes in the words or a serial order in the numbers, etc. But these difficulties are in large measure obviated after a little experience and practice in arranging the material to be presented. For this, and other reasons, the experiments which I made to measure the span of attention were uniformly of longer duration in exposure, and of more varied and richer content in impression;—tachistoscopically speaking, they are less of a 'snap-shot' and more of a 'time-exposure' than those classical experiments from which we have been quoting.

The method best adapted to detect individual differences in the scope of attention must be one which enables the attentional processes to act in as normal a way as is possible in laboratory work and which gives the experimenter a comprehensive view of what these processes are. Such a method, I believe, is, at least, approximated in the work now to be described.

Three series of experiments were made extending over several months and giving each subject a sufficient number of trials to enable him to express in averages his characteristics. Any one lot of experiments, taking the subject for one hour only, would not in all probability, tell a true story. I recall one subject whose results were fully 20 per cent better at one time than at another. It was a very extreme case; but the condition of the subject, if fatigued, nervous, confused by the experiment or what-not, will affect the results of his

work. This objection is done away with by the number of experiments and the wide variations in time between them.

As Dearborn found it necessary to vary the style and subject matter in his reading tests, so I found it necessary to vary the classes of objects exposed. As we shall see later, the sensory and motor elements entering into attention vary in their proportions for words and colors. With colors a visual memory image usually persists; this seldom occurs for words. Were the tests made for one class of exposures only we should probably have errors creeping in from differences in ideational type. To escape this I have taken one series of experiments with the attention upon words, one with the attention upon colors, one with the attention upon figures and letters, and I have used the sum of these results to represent the Span of Attention for the several subjects.

It became apparent early in the work that a time exposure of three seconds was too long for many of the subjects. One thing inhibited another and there was always a feeling that much more had been attended to than could be reported. It also became as quickly apparent that a half of a second was too short a period. For in this class of experiments the eye had no fixation point and would usually catch two or three objects, but would fail to survey the entire field. It was merely a matter of where the eye chanced to rest that determined the class and number of objects. After trying several fractions of a second it seemed advisable to use exactly one second's time exposure. This enabled the subject to survey an entire card, and did not permit him to overload his mind with observations which could not be retained. It also seemed to be an opportune time for preventing the grouping of objects, such as occurs with long time exposures. Associations would occur occasionally, but not frequently. Indeed, five monosyllabic words were frequently given without the subject forming any idea of their meaning, until they were reported.

At first the subject was requested to write all that had been observed. This had the effect of driving many things out of mind for several subjects, especially those whose memory

images did not seem as clear or of as long duration as the majority of subjects. So the plan of giving a verbal report was adopted, which entirely altered the results for several subjects. More in detail the experiments were as follows:

In series No. 1, (see Table 1) 50 exposures were made of 10 colors, upon the regular cards, with the regular one second exposure. The colors were shown to the subject before the experiment began, in order to familiarize the subject with the kind of color to be exposed and to learn his nomenclature for colors. All that was insisted upon was a sufficiently clear report, to make certain that the colors named had been actually perceived.

In series no. 2 (see Table II) 20 exposures were made of cards containing 5 three-letter words and five colored letters or numbers. The attention was directed to the words in order to involve the speech-motor factor in attention. If other things than words were perceived they were allowed to count in the total of things spanned by the attention. The proportion of objects perceived which were not in the class attended to is exceedingly small; as we shall see later. The totals are, therefore, a satisfactory index of span for this class of work.

In series no 3 (see Table III) 20 exposures were made of the same class of cards as in series no. 2; but the attention was directed in this case to the colored letters and numbers. The purpose in this was to submit a class of objects to the subject's attention which were intermediate between the color-class and the word class. For it had been noted n previous experiments that the setting of the attention was introspectively different for these two classes of objects.

In Table IV the totals for these three series were given for each subject; thus, the number of colors 'A' perceived in the first series was 215, the number in the second series was 97, and in the third 206. The total number of objects attended to in the ninety experiments was 518. That is, 'A' attended to an average of 5.75 objects for each exposure. This figure or the total itself is the index of 'A' for Span in this class of experiments.

	TABLE I				TABLE II			
SUBJECTS	TOTALS	CR.	AV.	MV.	TOTALS	CR.	AV.	MV.
A..........	215	4	4.30	0.58	97	3	4.85	1.18
B..........	205	7	4.10	0.62	86	6	4.30	0.80
C..........	223	1½	4.46	0.62	70	10	3.50	0.90
D..........	161	9	3.22	0.55	96	4	4.80	1.02
E..........	222	3	4.44	0.44	106	2	5.30	1.13
F..........	141	10	2.82	0.40	72	9	3.60	0.55
G..........	213	5½	4.26	0.57	74	8	3.70	0.93
H..........	199	8	3.98	0.46	88	5	4.40	0.88
J..........	213	5½	4.26	0.62	84	7	4.20	0.90
K..........	223	1½	4.46	0.69	125	1	6.25	1.14

	TABLE III				TABLE IV		
SUBJECTS	TOTALS	CR.	AV.	MV.	TOTALS	CR.	AV.
A..........	206	1	10.30	2.04	518	2	5.75
B..........	137	8	6.85	1.44	428	7½	4.75
C..........	169	5	8.45	1.74	462	5	5.13
D..........	122	10	6.10	1.04	379	9	4.21
E..........	185	2	9.25	1.64	513	3	5.70
F..........	123	9	6.15	1.25	336	10	3.73
G..........	141	7	7.05	1.12	428	7½	4.75
H..........	166	6	8.30	1.60	453	6	5.03
J..........	172	4	8.60	1.30	469	4	5.20
K..........	182	3	9.10	1.62	530	1	5.88

C.R. = Correlation Rank.
Av. = Average.
M. V. = Mean Variation.

From these tables it appears that there are large individual differences between subjects in their span of attention. Thus, 'K' has half again as large a span as 'F'; though several have span results quite close together 'B' and 'G' having the same totals.

In the table of correlations it will be seen that Table III correlates with Tables I and II, but that Tables I and II do not correlate. This shows there is a sufficient difference between the word-class and the color-class to affect the attention quite differently. It also shows that the experiments are successful in measuring a characteristic spanning of the attention; for the first series and the third, between the perform-

ance of which several months elapsed, have the correlational coefficient of 0.48.

Another evidence of the accuracy of the experiments appears in the tables for averages and mean variations from the averages. It is not possible to find any Probable Error in these measurements for an unusually large or small result does not necessarily indicate an error of any kind. The value of the experiments would be greatly reduced, however, if each subject varied very greatly from his average, for no figure would than represent his span accurately enough to permit comparison with the others. When the averages in the tables above are compared with their mean variations it will be seen that the variation is but a small part of the average (usually a sixth, occasionally a fourth). So the average is amply representative of the subject for comparison. The average is not given in succeeding experiments as it was found that the actual totals of results give a more accurate ranking for correlations.

The correlations for the above experiments, as they compare with each other and as they compare with the other experiments of the thesis, are all given. A separate table is also given of those correlations which are sufficiently above the Probable Error, for this method of correlating, to indicate significant relations. The method of correlating is discussed in the opening paragraphs of Part III.

CONCENTRATION AND INHIBITION

Attention has been regarded as simply the focussing of consciousness, which resulted in a consequent brightening of all it encompassed without directly affecting other portions of consciousness. It has also been regarded as the processes by which all other parts of consciousness are obscured except that which is directly engaged in perception or ideation. There is surely virtue in the 'golden mean' in this case. For that sharpening between the area of clearness and its penumbra of obscurity, which was mentioned in the last chapter, is much better understood if we consider both processes as complementary factors in attention.

Probably the sharpness with which the clearness area drops away into the obscurity area differs with different individuals. The introspection of Külpe, however, appeals to me as being in close concord with the average type of attention. He sums up his views:

When we ask how the degrees of consciousness are related to one another we find not one uniform graduation from the highest to the lowest, but in most cases a fairly sharp line of distinction. Certain contents stand at the level of clear apprehension; and from them our consciousness drops away; without transition, to the level of obscure general impression, above which the other contents of time are unable to rise. And the clearer the first group, the more indistinct are all the rest.[1]

Physiological psychology gives additional weight to this view.

Striking effects of concentration upon any object are frequent in the experience of everyone. There can be no doubt that we must find a physiological expression for this singleness of the object of attention and for the power of one object to banish all others from the focus of consciousness. Translated into physiological terms it means that only one of the perceptual systems of the cortical paths, consisting of one or more sub-systems of sensory areas united by higher-level paths, can be active at any one moment, that the spread of the nervous excitement through one such system somehow brings about the cessation of activity in the system active at the previous moment and prevents the activity of the other systems. Hence we need not seek for inhibitory centers in the cerebrum. Each perceptual system of arcs is an inhibitory center for every other, the activity of each system brings about as a collateral effect the inhibition of all others. . . Though we do not know how this inhibition is brought about, it may be conceived as a drainage of the free nervous energy from the inhibited to the inhibiting system, owing to the latter becoming for the moment the path of least resistance. There is evidence that similar inhibitory effects are excited by the activity of any one group of arcs of sensory area of the cortex upon other arcs of the same area, especially in the case of the visual area.[2]

Such a line of demarcation between the fields of attention and non-attention enables us to determine with considerable

[1]Titchener, E. B., *The Psychology of Feeling and Attention*, p. 222.
[2]McDougall, W., *Physiological Psychology*, pp. 102, 103.

accuracy the ability of the individual to concentrate his attention. From the testimony of those whose introspection resembles that of Külpe, the more intense the application of the attention, the plainer appears the inverse proportion in clearness between the attended and the non-attended. From the results of such work as McDougall's and Sherrington's, it appears that the more alive the one perceptual system of a sensory area the less alert is its neighbor. The concentration value, therefore, appears in the relation between the included and the excluded. In order to bring out individual differences fairly the cards containing five words and five colored letters or figures were exposed for one second in several series of experiments extending over five weeks. In the first series the subject was directed to concentrate upon the colored objects and to ignore the words The success with which this was accomplished was astonishing. The introspection was quite uniform. After the subjects had become accustomed to the work (and not till then were any of their results recorded) the ability to select one class or the other was very marked. For all subjects I have exposed the same card directing his attention now to the words and now to the colored figures. In not one instance did the subject know he had observed the same card—so completely were the objects, of the group not attended to, inhibited. When it is remembered that there were only ten objects before the subject and that they were exposed for an entire second the phenomenon of the inhibition of half of them becomes impressively significant. There was, however, less ability to get clean-cut results when the attention was directed to the words. The reason for this is the greater difficulty in attending words (compare Tables I and II for Span). It is far easier to catch a color when the attention is upon the word-class than it is to catch a word when the attention is directed to the colors.

The table below represents the subjects by the capital letters. The first row of figures represents the number of words which entered attention when the concentration was upon the colored figures, in a series of 25 exposures of the usual cards containing five words and five colored figures.

The second row of figures represents the colors, figures and letters which entered the attention when the concentration was upon the words. Twenty exposures were made for this latter series. In both series the exposure was one second. The totals give the correlation ranking.

TABLE V

Cor. R.	(4)	(6)	(9)	(4)	(7)	(4)	(2)	(8)	(1)	(10)
Subjects	A	B	C	D	E	F	G	H	J	K
1st Series	2	1	1	0	0	1	2	5	2	0
2nd Series	5	8	23	7	13	6	1	10	0	28
Totals	7	9	24	7	13	7	3	15	2	28

I cannot believe that these totals tell us very much about the ability to concentrate and inhibit as the processes normally operate. For it would appear that 'D' is four times as apt as 'K' in concentration, whereas 'D' was suffering from a nervous depression and could hardly keep his attention on his work for fifteen minutes, when the second series was taken. While 'K' was in excellent condition, a scientist with powers of concentration trained by many years of exacting work.

The figures may not be without significance for a study of differences which relate not to general conditions of mental energy, in concentrating, but to the more minute cooperation of the 'perceptual arcs,' which need not be a series of acts protracted over a considerable period. So that an attention which could not maintain its application for any length of time could still do a very clear-cut piece of work for a few seconds. Thus the above figures tell nothing of those great types of attention which Stern described.[1]

The effort to elaborate these experiments by introducing distractions was also rather abortive of results along the line of individual differences. In fact the work with distractions is rather misleading.[2]

[1]Stern, L. W., *Ueber Psychologie der individuellen Differenzen*, Kap. viii.
[2]Hamlin, A. J., Attention and Distraction, *Amer. J. of Psychol.*, viii, p. 3.

With one subject the so-called distraction may act as a stimulus, with another it may be very disconcerting when he is tired and nervous, but practically ineffective when he is at his best.

In one set of tests the room was lighted by electricity and for the cards upon which the usual five words and five colored objects were displayed, I substituted brilliantly colored papers of just the same proportion as the cards to serve as backgrounds. There was sufficient strangeness in the change from daylight to electric light to make the exposure seem a little different from usual, and so to discredit any shock which the first colored background might cause. There was nothing, however, to prevent the subject from detecting the change in background as the successive exposures were made, for even in the electric light the difference between the colored backgrounds was startling, when they were placed together. The eye, of course, had to traverse the colored papers in passing from one word to another. The concentration was upon words. Table VI, below, shows the total number of words observed in the seven exposures for each subject and the number of colored squares, letters and figures which were not inhibited. The lowest row of figures shows which background was first detected by the observer. It will be noticed that the first four backgrounds, which consisted of yellow, a pea-green, a light brown and a light blue, were inhibited by all subjects.

Seven exposures were given with the usual time, one second; the usual signal of one and one-half seconds; the usual objects were presented, in fact everything was carefully arranged to give the experiments the same setting as the many series which preceded them.

TABLE VI

	A	B	C	D	E	F	G	H	J	K	L
Words	29	25	20	26	29	24	23	25	25	24	28
Letters and Figures	0	2	0	0	1	1	0	1	1	6	1
Colors only	0	0	0	0	1	1	0	0	1	4	1
Backgrounds (number)	5	6	6	5	6	6	5	5	0	5	0

It would not be safe to draw any far reaching conclusions from seven experiments. It must, however, be obvious from the capacity of two of the eleven subjects to inhibit the change in background entirely that McDougall's surmise as to the inhibitory capacity of certain perceptual systems in the same sensory area is well grounded.

A comparison of the number of words attended in these seven experiments and seven similar experiments under usual conditions shows that distraction did retard many of the subjects. This a number of them felt during the experiment, but none of them assigned the influence to the right cause. Table VII shows in the upper row of figures the number of words attended to in seven experiments with no distraction, the lower row of figures shows the totals for the seven distraction experiments.

TABLE VII

	A	B	C	D	E	F	G	H	J	K	L
Cor. R.	(5)	(7½)	(1)	(9)	(7½)	(2½)	(10)	(6)	(4)	(2½)	Not used
	33 29	31 25	23 26	33 26	35 29	25 24	31 23	30 25	27 25	25 24	28 32

That the distraction affected the several subjects differently is obvious. (Rank for correlations is found by subtracting the lower from the upper row.)

The auditory distraction did little better service than the visual. This is to be expected. However, they seemed preferable to such distractions as require a distribution of interest: for in such work it is hard to distinguish between distraction and an actual division of attention.[1]

In the following experiments a fire-alarm bell was placed on a tin box within eighteen inches of the subject's ear. An electric attachment rang a bell the instant the shutter of

[1]Darlington, T. and Talbot, E. B. Methods of Distracting the Attention. *Amer. J. of Psychol.*, ix, pp. 332-345. Also, Moyer, F. E., *op. cit.* viii, p. 405.

the tachistoscope opened, and broke the circuit the instant it closed. The noise was violent, almost intolerable to the experimenter; but in many cases it was not found objectionable to the subject engrossed in seeing the exposure. Ten experiments were made under the usual conditions of time-exposure, signal, number and character of objects exposed. The following table shows how many words were attended under the distraction conditions and how many were obtained in a series of experiments without distraction, which were chosen at random from another series. (Correlation-rank is found by subtracting the Distraction from the Non-distraction row.)

TABLE VIII

Cor. R.	(10)	(5)	(4)	(2)	(9)	(1)	(8)	(3)	(6½)	(6½)
Subjects	A	B	C	D	E	F	G	H	J	K
With distraction	23	39	32	33	29	38	29	39	36	30
Without distraction	33	40	32	31	37	32	36	38	41	35

It appears that three subjects 'D', 'F' and 'H' did better with the fire-alarm than without it, and that 'C' found its presence indifferent.

If such an experiment could be varied to prevent accommodation on the part of the subjects, and could be tried a great many times on many subjects it might bring out an interesting typical trait in some subjects; namely, that the reinforcement of those sensori-motor arcs engaged in perception is conditioned by such a general agitation. Physiologically it seems to point to typical differences in the ability to adjust the great afferent currents to their appropriate motor discharges, and so brightening the vividness and the clearness of the attention.

A rather difficult experiment was made to discover, if possible, what differences of attention might appear if not perceptual matter, but conceptual matter, were material for concentration and inhibition. As Professor James says:

The immediate effects of attention are to make us perceive, conceive, distinguish, remember better than otherwise we could—both more successive things and each thing more clearly .[1]

So the facility in discriminating and retaining may be considered an indication of the ability to concentrate. Upon this principle a series of fifteen experiments were made. Each card was exposed three seconds, displaying ten words. Five of these words were related to each other. Thus, some were names of parts of the body, or parts of a house, articles of furniture, kinds of animals, of colors, of fruits, etc. All words on any one card were of the same length. Nothing in the appearance of a word would indicate whether it were of the class to be attended or rejected. They were also thoroughly mixed together so that the eye had to traverse the card to observe all those of a certain class. Each subject was carefully instructed about the work and was told a few seconds before the shutter opened what class of words to seek.

The introspection was of a very similar character in all cases in which it could be given. Two subjects found that they could not recall how certain words were retained and others inhibited. It would seem that the unsought words are perceived, but scarcely recognized as of the unsought class, and instantly dropped. Probably they were not thrust from attention, but their memory is erased by the incoming correct word. This obliteration, or lack of assimilation, when one thing follows closely upon another is a familiar phenomenon in consciousness. Enough unanimity appeared in the introspection to make it evident that the eye tarried longer upon the desired classes than upon the others. This, too, would add to the process of inhibiting. In a very large number of cases the words of the unsought class would not be recognized by the subject when read to him. Indeed many cards were shown to the subjects when they had made unusually good records, and they declared that they could not have seen the words of the unsought class for not a vestige of them remained in consciousness.

[1]James, W., *Psychology*, vol. i, p. 424.

The following table gives the total number of words attended to in the sought classes and the total number of those attended to in the unsought groups:

TABLE IX

Subjects	A	B	C	D	E	F	G	H	J	K
Words correctly attended	56	61	47	59	63	56	61	64	62	55
Words which should have been inhibited	29	16	14	12	16	18	14	21	11	9

If the figures for the words which were correctly attended to by each subject are used as numerators and those which should have been inhibited, as denominators, then the quotients will serve for correlation ranking:

	A	B	C	D	E	F	G	H	J	K
Cor. R.	(10)	(6)	(7)	(3)	(5)	(8)	(4)	(9)	(2)	(1)
	1.93	3.81	3.35	4.91	3.93	3.11	4.35	3.05	5.63	6.11

The above tables do not correlate with each other in a single instance. (See Correlation Tables). That is, each series of experiments tells a different story from the others. So no conclusions can be drawn concerning Concentration and Inhibition as a typical mental trait. The experiments show, only, marked individual differences in the subjects under the special conditions of each experiment.

The Mean Variations in the experiments on Span show individual differences in the constancy of attention. It varies in its efficiency so that one Mean Variation is occasionally half again as large as another. But here, again, there is absolutely no correlation between the series. The fact of variation in constancy of attention must be recognized along with the facts of variation in concentration, though they may not be shown in their relation to other mental traits.

MOBILITY OF ATTENTION

From the preceding experiments it seems clear that one class of exposures is easier for certain subjects to attend than others, from which it might be assumed that each subject would quickly choose his best class. But a series of twenty-six experiments, which were performed when the subjects had been working with the tachistoscope a few times, reveals a different situation. For in this instance the subjects were told to get all of the objects presented, if possible. They immediately tried to attend to all the words first and then the colored figures. This continued throughout the series. Subjects, who later proved that colored figures were more easily obtainable for them, in this early experiment stuck to the word list all through. The explanation is not far to seek. No matter what class of objects the attention is considering it is more natural to continue in that class than to shift to any other. The subjects were disposed to think that words were the easiest to attend to and that bias started them on the words lists. Once started upon that course they staid in it through many experiments.

Introspectively, it seems obvious that the attention stays upon one class of perceptions in preference to changing. Fechner noted this many years ago. It has often been corroborated. Every day's experience bears witness. The turning from the newspaper to composing a letter, the changing of attitude in passing from social to business affairs, the shifting of thought in passing from one picture to another in a gallery; these and a host of experiences give evidence of the 'inertia of attention.'[1]

The explanation of this lack of agility in attention is to be sought in that setting of consciousness which results in adjustment of end organs, nervous system and brain paths, to receive the sensation expected. Organic adjustment, then, and ideational preparation, or perception are concerned in all attentive acts. As Wundt says:

[1]Titchener, E. B., *Psychology of Feeling and Attention*, p. 246.

Every idea takes a certain time to penetrate to the focus of consciousness. And during this time we always find in ourselves the peculiar feeling of attention. The phenomena show that an adaptation of attention to the impression takes place.

Of course this has been apparent since Wundt's experiments upon reaction-time and attention; and especially since Münsterberg's more complicated reaction experiments which showed that a portion of consciousness could hold itself in readiness to do certain work at a certain time. This was also evident in those experiments in which the attention was prepared to get a certain class of words exclusive of others, (described in the last chapter.)

Such preparation of the perceptual and conceptual systems argues that certain percepts and concepts have certain courses through consciousness, that there are definite adjustments in the complexes of sensori-motor paths. So several of my subjects would say, "Now I am going to attend to this exposure with motor attention," or sometimes "with purely visual attention." They were conscious of a different setting in each case. For the colored objects the attention was largely visual, for the words it was largely motor,—in many cases the subjects repeated the words *sotto voce*. The figures and letters were sometimes attended in one way sometimes in another. The point is, however, that for the different classes of objects, different kinds of acts of attention were required. The change from one class to another demanded a change of attentional attitude. This we shall discuss more fully in a later chapter. It is sufficient to state the mere facts in the matter.

The ability of the attention to change from one to another class shows an interesting trait in the control of the attention. To bring this out clearly I arranged fifteen cards with colored geometrical forms, colored figures and letters, and monosyllabic words. These were placed on the cards in miscellaneous positions and in widely varying proportions. It was impossible to set the attention for any one class. It was necessary to change swiftly from class to class in order to obtain several objects in the one second exposure allowed.

The table shows the per cent of objects attended to the total number of objects displayed in each series.

TABLE X

Cor. R......	(2)	(3)	(1)	(8)	(10)	(7)	(5)	(6)	(9)	(4)
Subjects.....	A	B	C	D	E	F	G	H	I	K
Percentage...	56.4	53.5	59.9	42.4	40.8	44.5	52.0	48.5	42.0	52.1

So it appears that between 'C', and 'E' there is the greatest difference in mobility of attention. 'C' is nearly half again as apt as 'E' in switching the attention from one class to another.

Here, as in Concentration, *we must remember that these processes which occur in the fraction of a second may not be descriptive of all those activities which take a longer time and are more natural.*

CELERITY OF ATTENTION

That there are differences in the rapidity with which the central processes prepare to attend certain stimuli is amply demonstrated by those experiments in reaction-time where a signal is given at varying intervals before the stimulus is offered. Some individuals react better to a two-second warning, some to one second; the average preferring one and one-half seconds. So, too, we have seen that the rates of rapidity of attentional acts vary greatly in different readers. This difference was thought by Dearborn to correlate with breadth of span in attention, "The slow readers have a narrower span or working extent of attention."[1]

It seems well worth comparing some experiments which bring out the subject's attitude in attending to a number of objects in quick succession and a series which indicates a span of attention.

The following experiments were performed in the spring of 1908 with different subjects from those whose results are

[1]Huey, E. B., *The Psychology of Reading*, p. 178.

given in the other experiments. Let us call the six subjects 'X,' 'Y,' 'Z,' 'W,' 'A,' 'D'. 'A' and 'D' are the same subjects represented by these letters in the other experiments.

To bring out the rapidity with which the attention could pass from one subject to another, about thirty monosyllabic words, thirty black geometrical forms and thirty small colored squares were arranged in parallel columns. (A different form of tachistoscope was used from that described above. It allowed a very large field of exposure.) The subject was told to count the objects present as rapidly as possible. Three-second exposures were given. Then he was directed to write down as many objects as he could remember. The recalled objects were remarkably few in view of the fact that the subjects had seen these same words, forms and colors in other experiments many times. This phenomenon of inhibition does not interest us here, except that it shows the rapidity with which the counting was done. The remembred objects were totalled and presented in the lower row of figures in table A.

TABLE A

Cor. R.	(1)	(6)	(3)	(5)	(2)	(4)
Subjects	X	Y	Z	W	A	D
Total Objects seen	77	49	54	51	57	52
Total objects remembered	11	9	8	14	11	7

In this series it appears that X is half again as quick as Y in control of his attention.

This experiment was supplemented by one in which the subjects counted eighty objects and their time of observation was carefully taken. The following table shows the time required.

TABLE B

COR. R	(1)	(4)	(3)	(5)	(2)	(6)
Subjects	X	Y	Z	W	A	D
Number Seconds	18	25.6	20	26.8	19.6	27

When these two experiments are compared they both tell the same story as we shall see later.

To get the span of attention a photograph was exposed for ten seconds and the subjects wrote down all the details they had attended to. This was done with a colored picture also. Then a series of exposures of three seconds was made for ten cards containing eight to ten colored letters and geometrical forms. The following table gives the number of objects attended to by each subject and the totals.

TABLE C

COR. R	(1)	(5)	(2)	(4)	(3)	(6)
Subjects	X	Y	Z	W	A	D
Picture	54	11	29	22	18	18
Colored Picture	54	12	23	19	22	18
Cards	87	55	66	63	76	31
Totals	195	78	118	104	116	67

In addition to the experiments which sought to determine the span of attention for visual perception a large number of trials was given each subject for auditory span ('umfang'). An instrument clicked uniformly for several minutes. The subject was instructed to group the clicks, without counting them, in the largest numbers possible. This was indicated by the subjects raising the hand or tapping with a pencil when a group was completed.

The following table shows the groups to represent the subject's average 'span'. The figures represent the number of clicks to a group.

TABLE D

COR. R	(2)	(6)	(1)	(4½)	(3)	(4½)
Subjects	X	Y	Z	W	A	D
Groups	14	4	16	8	12	8

If these four tables are correlated according to Spearman's 'Footrule' for correlating, which is explained later,[1] the following correlations appear.

[1]See page 47 for explanation of the 'Footrule.

TABLES	A	B	C	D
B	0.67		0.67	0.69
C	0.48	0.67		0.56
D	0.56	0.69	0.56	

The probable error is .176.

From these figures it is very evident that, under the conditions of our experimentation, there is an intimate relation between the ability to shift the attention from one object to another and the ability to grasp a large number of the objects. That this celerity and spanning of the attention are not merely a peculiarity of attention in visual perception is proved by the high correlation between the auditory 'umfang' and the results for celerity.

The results of some experiments designed to detect the relation between span and association time, performed at the Princeton Laboratory, should be noted here. The span of attention was obtained as follows. Cards with five monosyllabic words upon them were exposed for two seconds, in *Series A*, of twenty tests. While the subject was attending to the words a series of sounds (four on an average per experiment) were made by striking iron on wood, on tin, on a gong, on a steel rod, by a buzz, a series of tones on steel bars, etc. The subject sought to attend to all of the words and all of the different sounds. The scheme was reversed in *Series B*, of twenty experiments. Here the subject put his attention primarily upon the five words which the experimenter read aloud while six colors or colored forms passed before the eyes in the second during the reading. Obviously, the attention was strained to its uttermost. The total number of different sounds heard and of words seen in the first series for the two second exposures served as an index of span of attention. The second series, with the attention primarily upon the words spoken, but seeking to grasp all the colors and figures seen, supplements the first series and gives a further value in its totals for span.

Each of these series was compared with the subjects'

association time for one word and for four words. The following correlations resulted.

EXPERIMENTS COMPARED	COR. COEF.	PROB. ERROR
Span in Series *A* and Ass. Time 1 word	.334	.16
Span in Series *A* and Ass. Time 4 words	.334	.16
Span in Series *B* and Ass. Time 1 word	.429	.16
Span in Series *B* and Ass. Time 4 words	no correlation	

These results show that there is *probably* a connection between the ability to span many things in a short period and the ability to associate quickly. However, such a correlation as that which appears between the span of attention for spoken words, colored objects, etc., and association time for one word is very suggestive, when considered in the light of the results for span and quick shifting of the attention in perception. Here the connection lies between span and the quick shifting of the attention in conceptions.

SOME MEMORY FACTORS IN ATTENTION

A very noticeable difference appeared among the subjects, throughout the entire work, in the way in which the objects attended to in one experiment would lie dormant in the mind and would be reported as seen in a later exposure. By what unconscious or co-conscious process this was done no introspection could discover. In order to throw some light upon the phenomenon four sets of experiments were made. In the four sets the same cards were used. These contained 5 three-letter words and five colored letters or figures. The exposure was one second. In each set the subject was instructed to attend to the word-class in a first series, and later to attend to the color-class. In both cases he was to perceive as many objects of the class attended to as possible, but was to report *everything* he observed. In each set a number of cards were repeated, after the lapse of a certain period, to discover whether any details would appear in the second exposure which were not reported

in the first. If the influence of an exposure lingered in the memory, it should, of course, make itself evident in some difference between the repeated cards and those which were presented for the first time.

Proceeding upon this hypothesis, about twenty cards were repeated after the lapse of a week. There was nothing whatever in the results of any subject to indicate the influence of previous experiments. The results for cards never seen before were quite as large as for the repeated cards. There were no introspective results to indicate a continuance of the previous objects exposed in memory.

It was impracticable to vary the number of days between the experiments, so the next set of trials was made within an hour's experimentation. In order to repeat a sufficient number of cards in the hour a period of five minutes was allowed to elapse between the original exposure of a card and its repetition. In Table I five minutes elapsed between the first and the second exposures of the cards. In Table II there was a two minute intermission, and in Table III the card repeated followed immediately upon its first exposure. The figures in the tables are found by subtracting the results for the first exposures from those of the repeated exposures, then dividing by the number of experi-

	Table E		Table F		Table G			
	W	L	W	L	W	L	Totals for Subjects	Cor. Ranks
A	0.90	0.11	−0.80	1.22	0.75	2.50	4.68	(6)
B	0.75	0.83	0.80	−0.25	0.00	1.50	3.63	(10)
C	0.07	1.50	1.33	1.80	0.60	2.00	7.30	(2)
D	0.54	1.44	−0.16	0.50	0.40	1.80	4.52	(7)
E	0.44	2.00	0.40	0.20	2.00	1.80	6.84	(3)
F	0.27	1.00	0.33	1.25	0.25	1.80	4.90	(5)
G	0.60	0.62	0.60	1.33	1.00	0.20	4.35	(8)
H	0.45	0.33	0.40	0.00	1.25	1.75	4.18	(9)
J	0.10	0.83	0.00	2.00	0.75	1.30	4.98	(4)
K	1.00	0.87	−0.16	2.16	1.70	2.30	7.87	(1)
	0.512	0.953	0.274	1.021	0.870	1.695	Averages (for experiments)	

ments per subject. Where the subject averaged less per experiment in the repeated than in the original exposures the loss is indicated by the minus sign. ('W' = word-class, 'L' = letter-class).

The difference between the subjects is very evident from the tables; there is, however, no correlation between the subjects' averages.

In noting the greater increment in repetitions for the word-class than for the letter-class it must be remembered that the Span itself is greater for the letter-class.

The most instructive feature in the Tables is the increase in the averages for the repeated exposures. This clearly proves that something of the first perceptions of the cards lingered in memory and assisted in later observations. This influence became more marked as the time between the original and its repetition was shortened. In the repetitions which followed immediately upon the originals the cards were frequently recognized after the subject had seen two or three objects, but in the series of repetitions following two and five minutes after the originals, recognition was comparatively infrequent.

IDEATIONAL TYPES

The terms 'Memory Types,' 'Speech Types', 'Types of Imagery', have been used very frequently to point out characteristic differences in the way in which different classes of individuals imagine, remember, and speak.

In both physiological and theoretical psychology the 'division of labor' in the mental economy is constantly emphasized. Experiments are often performed to show that memory is not a faculty dealing with all matter in the same way, but that it divided its labor among visual, motor, auditory and other functions of the mind. The work upon aphasia has been especially enlightening.

> Internal speech is a revival of auditory, visual and articulatory memories, its integrity depends upon the united action of these three centers; but the one which is the most highly cultivated is revived most vividly.[1]

[1]Collins, J., *The Genesis and Dissolution of the Faculty of Speech*, p. 62.

Charcot and his school referred to those who were the most proficient in any one faculty as 'visuels,' 'auditifs,' and 'moteurs.' Such proficiency comes from either a natural bent or an adaptation of certain faculties to certain work. So Baldwin states:

The brain is a series of centers of relatively stable dynamic tension, the various associative connections among these centers are paths of less and more rather than least and most resistance; the range of alternative judgment is occasionally wide, and consequently any individual has his "personal equation in all functions as complex as those of speech. One man is a 'motor', a second a 'visual', a third an 'auditive', according as one or another of the extrinsic causes of stimulation suffices to release the necessary energy into his motor-speech center."[1]

Not only are the activities of these complexes, that give rise to expression, conditioned by the brain paths which nature designed as the highways for her nervous currents; but, also, by those byways which much traffic has developed into highways. Could we make blue prints of these courses our charts would show some strikingly characteristic differences. And we should be thoroughly prepared to believe that the conduct of consciousness in general is obliged to adapt itself to the conditions of its thoroughfares. Certainly it would seem most probable that the "area of greatest clearness" in consciousness would correlate with the broader and more evident mental types. We find, on examining consciousness, that attention is not a fixed thing, a faculty, any more than are memory and imagination.

Yet in much of the literature of late years, in which the faculties have been scouted, I know of no author who has applied his own criticisms consistently to the attention. Memory on the other hand is now known to be a function of the content remembered, and not a faculty which takes up the content and remembers it. So we have no longer one memory, but many, visual, auditory, motor memories. Yet the very same thing is true of attention. We have not one attention but many. Attention is a function of content; and it is only as different contents

[1]Baldwin, J. M., *Philos. Rev.*, July, 1893, p. 389.

attended overlap and repeat one another that they have somewhat the same function in attention.[1]

The problem before us is to discover whether these ideational differences actually do affect the attention. Does a strongly visual type naturally select a different class of objects to attend to from those that appeal to the motor type? Is it easier for those perceptions which call out motor activities to be attended to by the motor type than by the visual? Do such differences stand out clearly, or are individuals now of one and now of another type? These and similar problems are now before us.

It will, perhaps, make the work appear clearer if the experiments upon types of imagery are given first. It will enable us to detect those subjects who are of a pronounced type and we may follow their results through the other experiments readily.

Galton, who with Titchener, is one of the pathfinders in mental types, prepared a table of questions whose answers indicated the subject's type.[2] These answers were graded according to the degree of the subject's realization of the imagery. If it was very clear, bright and distinct it headed the series; if no image at all was realized, it was graded zero. Between these extremes were seven intermediary grades. In general, I followed Galton's scheme. With this difference, however, that each subject graded himself according to the clearness and vividness of his imagery upon a scale of ten; zero being the lowest and ten the highest. The questions were read to the subjects and any misunderstandings were made clear. Ample time was allowed for deliberation. The subject considered a visual image first, then auditory, then motor. This enabled him to make frequent comparisons of the three orders and allowed for a separate effort and judgment for each question. The experiments continued through several weeks. This tended to eliminate minor differences.

[1]Baldwin, J. M., *Mental Development*, p. 468.
[2]Galton, F., *Inquiries in Human Faculty*.
Titchener, E. B., *Experimental Psychology*, vol. i, chap. xii;

Subjects varied in their abilities from time to time. Thus 'K' who was very good in introspection found that certain kinaesthetic images would vary perceptibly. 'D' was unusually poor in his introspection; so inefficient, in fact, that all images seemed equally distinct whether visual or gustatory, auditory or olfactory. I have inserted his results for a similar series of experiments performed in the spring of 1908, when he was in better physical and mental condition. I found that the memory images of nearly all of the subjects were conditioned by the memory of the particular person or the particular thing remembered. To acquire a greater uniformity I selected purely imagery matter for consideration.

The questions were for the auditory imagery: "How clearly can you hear a harp and flute playing together; a trio, of two males and a female voice; the wind blowing through trees and sound of waves; the wind blowing a tune; the songs of the Bedouin Arabs; a conversation in Arabic; the cry of camels; the scraping of a file on a violin; the sharpening of a saw; an artillery bombardment." For the visual were asked: "Can you represent to your mind an image of a pyramid; can you see the stones clearly; the cracks between the stones; the sand wastes around the pyramid; the skies above it; can you imagine a cobweb colored red; a battlefield; a printed page with every other line in colored ink; a giraffe reaching for the leaves of a tree." For the motor imagery the following were asked: "Can you represent to yourself how it feels to write with the left hand; to wind the watch with left hand; to throw a stone with the hand not usually used in throwing; to walk backward; to lift the hat with the left hand; to walk through sand shod in sandals; to do an oriental dance; to waltz and stoop at the same time; to gesticulate like the deaf and dumb; to ride upon a camel."

When a subject was left-handed or ambi-dextrous a question was substituted for those implying a normal right-handed practice.

The following tables represent the totals of the grades for the three orders. Questions were not asked concerning gus-

tatory or olfactory images after the lists for strictly memory images were discarded

SUBJECTS	TABLE XI		TABLE XII		TABLE XIII	
	VISUAL	C. R.	(AUDITORY)	C. R.	(MOTOR)	C. R.
A.	47	(2)	16	(10)	48	(1)
B.	39	(5)	22	(7)	31	(6)
C.	50	(1)	41	(2)	37	(3)
D.	27	(9)	49	(1)	33	(5)
E.	41	(4)	37	(3)	25	(10)
F.	23	(10)	25	(5)	27	(8)
G.	30	(8)	18	(8)	34	(4)
H.	38	(6)	33	(4)	30	(7)
J.	45	(3)	17	(9)	41	(2)
K.	36	(7)	23	(6)	26	(9)

These tables were supplemented by a series of experiments upon associations, to discover whether the visualizer differed from the others in the class of associations or in his association-time. The subjects were also given problems, from time to time, and sought to introspect upon their habits of mind in solving the different kinds of problems. The result of this work is not given as it did not prove satisfactory. The above table, which is based upon Titchener's method gave the clearest results obtained.

VISUAL PERCEPTION AND ATTENTION

Our perceptions are directed by the heritage of brain traits and experiences. When these mark us as visualizers, our perceptions will be different from those of the motor types. In the perception of simple color there is very little motor activity. Usually there is no disposition to pronounce the name of the color or shade and the effort to recall it is distinctly visual. This is not so of words. Though they, too, are sometimes recalled usually and perceived as visual objects without an introspectively perceptible reaction, when they occur, as a usual experience they are pronounced. Frequently this is merely an internal expression; very often

it results in lip-movement, and frequently in an audible though unconscious articulation. When the distractions accompanied the effort to perceive the largest number of words possible, there was a very marked increase in motor activity which in several instances resulted in the subjects speaking aloud though they were not aware of it at the time.

In the following experiment there were exposed fifteen cards containing colored objects, figures, letters and three-lettered words. The subject was told to select as many objects as possible regardless of class. The exposures were one second. Each subject had had a long and varied experience in perceiving the classes of objects presented and instinctively sought that which was easiest. This had to be instinctive, as each card varied so in the number of the several classes of objects and the time was so short, there was no opportunity for deliberation.

In the following table the per cent of colors obtained, to total number of colors, is given:

TABLE XIV

Cor. R.	(4)	(2)	(1)	(8½)	(5)	(10)	(6)	(8½)	(7)	(3)
Subjects	A	B	C	D	E	F	G	H	J	K
Percents	42	54	58	30	39	26	38	30	31	51

The high figures for several subjects whom we saw classed as visualizers in the ideational type experiments is practically explainable by the fact that they have a very wide span of attention for the color classes, as may be seen by comparing their results for 'span,' and they would get large results in such cases. We shall speak of this later.

Another experiment involving color, and used in another connection, will assist in the present problem. Twenty-six one-second exposures were made for cards containing five words and five colored figures. The attention was upon the colored figures primarily and the subject was to perceive both figure and color and associate them together in his report. This involves motor as well as visual factors. The total num-

bers of associated colors and figures are given for each subject in the table below:

TABLE XV

Cor. R.	(1)	(6)	(3)	(2)	(7)	(8)	(10)	(5)	·	(9)
Subjects	A	B	C	D	E	F	G	H	J	K
Totals	67	28	38	39	22	21	6	30	35	8

The totals of all the colors perceived out of a possible hundred and thirty for this same series are given in Table XVI

TABLE XVI

Subjects	A	B	C	D	E	F	G	H	J	K
Cor. R.	(1)	(7)	(2)	(10)	(3)	(8)	(9)	(5½)	(5½)	(4)
Total Colors	92	53	73	35	69	42	37	54	54	67

Another experiment was made in which five colors and five geometrical forms were presented in one second exposures. In all there were fourteen exposures with the attention directed to the colors which were simple and easily perceived and recalled. The totals for the colors attended are as follows:

TABLE XVII

Subjects	A	B	C	D	E	F	G	H	J	K
Cor. R.	(5)	(7)	(3½)	(9)	(6)	(8)	(10)	(2)	(1)	(3½)
Totals	62	54	63	42	55	43	27	64	66	63

One of the most interesting of these experiments was performed as follows: ten brightly colored squares were exposed for one second, the subject was to perceive as many as possible and report. After fifty such exposures were made five words were substituted for five of the colored squares. The change in the character of the cards was not announced until subjects discovered it themselves. All through the series the the instructions were to get all that it is possible to perceive

no matter what it is. The result was that some subjects continued to get colors and some switched over to the words. There was probably no fatigue, certainly not enough to count for any changes in the direction of attention. The choice of the word class after the mometum of fifty experiments in the color class is indicative of a natural preference for the word class.

In Table XVIII the totals for the colors in the first fifty experiments are given. And in Table XIX the per cent of colors in relation to total perceptions are given. (The former is Table I for Span.)

TABLE XVIII

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R......	(4)	(7)	(1½)	(9)	(3)	(10)	(5½)	(8)	(5½)	(1½)
Total Colors.	215	205	223	161	222	141	213	199	213	223

TABLE XIX

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R......	(1)	(6)	(4)	(7)	(5)	(9)	(10)	(3)	(8)	(2)
Percent of Colors.....	100	57	64	44	60	30	21	69	41	85

In the Table of Correlations a number of significant correlations appear between these experiments on the visual factors in attention. Thus, this last table correlates with tables XV, XVI and XVII. The meaning of this will be discussed in the Conclusion; here it is sufficient to call attention to the fact that this class of experiments do not correlate with a single one of the experiments in the next chapter designed to bring out motor factors in attention.

MOTOR FACTORS IN PERCEPTUAL ATTENTION

The common experience was that the words called forth the 'inner speech.' Each one was pronounced. At times the subject would pronounce the word 'Bug' as 'Rug,' and when

giving his report would repeat it as he pronounced it to himself, but it would seem wrong and after a little thought he would correct it by visual memory. This was not at all common. The motor-auditory process usually predominated, especially in memory. For it was a universal experience that the words meant nothing. They were as so many nonsense syllables until reported, and within two minutes after the report, they were forgotten. Without the co-operation of the 'inner expression,' the mere visual perception of the words would have been retained very poorly. From the experiments in reading it would seem that this inner speech is an incipient movement. It does not effect the larger or chest muscles perceptibly, nor does it produce lip-movements necessarily. It is a motor activity which apparently varies as the effort to make the perception clear and strong varies. It is marked in children learning to read. I have found it very pronounced when learning a strange language, but it is diminished as the language was acquired. Quantz thought:

> It is a specific manifestation of the general psycho-physical law of dynamogenesis by which every mental state tends to express itself in muscular movement.[1]

Our interest is to discover whether those who appear, according to their own introspection, to be clearly motor types are influenced in the direction their attention instinctively takes, in the effort to grasp as many objects as their span will allow, when these objects call out more or less of the motor activity.

To bring this out a series of fifteen exposures was given the subjects, of cards containing five words and five colored letters, numbers or figures. The subjects were told to make their own selection of class of objects, but that they must attend to the largest number possible in every case. In Table XX the per cent of words to the total number of objects attended to is given.

[1]Quantz, J. O., *Psychol. Rev.*, Mon. Supp., vol. ii, No. I. See also, *Philos. Rev.*, ii., pp. 385-407; *Psychol. Rev.*, 1894, pp. 441-453; *Yale Studies, Psychol. Laboratory*, ii. p. 122.

TABLE XX

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R......	(4)	(1½)	(8)	(7)	(9)	(3)	(1½)	(5)	(10)	(6)
Percent of Words.....	73	81	60	62	38	74	81	72	37	65

The differences in the results for the subjects are sufficiently large to show individual preferences which may be taken as indicative of mental traits as affected by this kind of experiment. What these traits are does not appear, for this series does not correlate with any other in the whole Table of Correlations.

After the lapse of several weeks another series of exposures was made in which the subjects were again directed to seek that class which is the easier to attend to. In this case there were five words and five colored figures. Twenty one-second exposures were made. The totals for word are given in Table XXI.

TABLE XXI

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R......	(8)	(10)	(5½)	(5½)	(7)	(3)	(4)	(9)	(2)	(1)
Total Words.	60	42	72	72	69	77	76	48	85	86

This table not only does not correlate with the precediny experiment of like character; but it also fails to correlate with any of the others except those represented by Table IX in Concentration and Inhibition. From this correlation it would appear that the disposition to turn the attention to words in preference to colored objects and the ability to set the attention for a certain class of words and to inhibit others, are faculties which go together.

In the next experiment the subjects concentrated upon the words only. They sought to inhibit all else. The purpose in this was to discover whether there were any correlations between the ability to attend to the word class and the other

acts of attention involving motor factors. There were six three-letter words and six colored figures on each card. Twenty exposures of one second each were made. The total number of words attended to are given for several subjects in Table XXII.

TABLE XXII

Subjects	A	B	C	D	E	F	G	H	J	K
Cor. R.	(8)	(2)	(9)	(7)	(3)	(10)	(5)	(4)	(1)	(6)
Totals	66	79	64	70	78	63	75	76	82	71

Here, the only correlation is with the experiments for auditory factors. In this case the high correlational coefficient of .51 indicates a relation between the ability to concentrate upon the word-class and the ability to attend to the reading of poetry while viewing the presentation of a succession of words. (See Table XXVIII in chapter on Auditory Factors.)

Table XVIII of the last chapter may be used to find the per cents of those who, having observed colors for fifty experiments detected the wordsand attended to them when they were added to the color-cards. The per cent of words to total number of objects seen,—when the words were added, is given in Table XXIII; which is Table XVIII of the last chapter reversed.

TABLE XXIII

Subjects	A	B	C	D	E	F	G	H	J	K
Cor. R.	(10)	(5)	(7)	(4)	(6)	(2)	(1)	(8)	(3)	(9)
Percent of Words	0	43	36	56	40	70	79	31	59	15

This series makes two interesting and important correlations with the experiments on Concentration. The first correlation is with the experiments which were designed to show the subjects' ability to concentrate upon words or colored objects to the exclusion of other classes of objects. The correlation here points to a relation between an aptitude for the word-class and the ability to expend the attention upon one

class exclusively. This hints at what has already been noted, that the greater activity of attention required by the word-class demands a greater effort; and, further, appeals to those to whom this kind of effort is natural and congenial. The second correlation is with the series of experiments in which the subject set his attention for a certain class of words before the exposure, and inhibited all of the other classes. Here the faculty of turning instinctively to the word-class and the ability to set the attention to perceive a certain class of objects appear related. This may mean that the motor factors which make it easier for certain subjects to attend to the word-class also make it easier for them to react to a word for which they are seeking. The efficiency then, in both cases would be attributable to a characteristic readiness of motor response.

AUDITORY FACTORS IN PERCEPTUAL ATTENTION

In the experiments upon aphasia it has been shown that the motor-auditory complexes play as important a role, in the processes of attention which accompany speaking and reading, as any other. The inner speech, of which we spoke in the last chapter is, according to Huey, a combination of motor and auditory elements, with one or the other predominating according to the subject's habitual mode of imagining.[1] Huey and Dodge both agree that the motor element is present with those who auditize in reading and that the auditory element is present with those who motorize. Huey believes that these factors may not be so prominently present with visualizers.

Now, our interest is to learn what differences characterize this motor-auditory type in their attentional processes. They should find those perceptions which can be the more readily dealt with by the habits and traits of their motor-auditive systems easier to bring into the 'area of greatest clearness' than perceptions which do not call out such reactions. We should be justified, in the light of experiments previously described, in performing a series of experiments in which the

[1]Huey, E. B., *The Psychology and Pedagogy of Reading*, p. 120.

competition between the visual-motor and the auditory-motor would indicate which was the more characteristic of the subject's type.

Such a series of experiments was undertaken as follows: the subject sat before a rotary tachistoscope in which several words were exposed clearly to view during each exposure. While the subjects saw and read these words two lines of a poem were read to them. The attention in the first series of experiments was directed to the words, but the subject was also told to retain as much of the poetry as possible. When the report was given, the words were recited first, then the poetry. The purpose in having the attention thus divided, rather than free to choose either class of perceptions, was this; it is impossible to present auditory and visual stimuli with such equality that the one does not obtain some advantage over the other. It is better therefore, to give an advantage first to one and then to the other by distributing the attention consciously, and placing its emphasis on one or the other. This would give the advantage first to one type and then to the other. This is what was done. After a series of twenty-three experiments (Tables XXVII and XXVIII) in which the attention was primarily upon the visual impressions, another series (Tables XXV and XXVI) of twenty-two experiments was made with the attention directed to the auditory impressions. The per cent of words to the total presented is given in the left-hand columns of both tables. The accuracy with which the poetry was attended to was graded with one hundred as the highest count. The results for poetry appear in the right-hand columns. Care was taken that the poetry should be simple and the lines of sufficient brevity to enable each subject to grasp it no matter how inefficient he might be in memorizing lines read aloud.

In Table XXV are given the words perceived when the attention was on poetry, and in Table XXVI the grades for the amount of poetry attended in the same series are given. In Table XXVII appear the word totals when the attention was directed primarily to words, and in Table XXVIII are the grades for the poetry in this series. Table XXIV gives

the sums of the grades for the poetry, and the totals for the words together, for each subject. These indicate the span of attention for visual and auditory perceptions combined.

TABLES	XXV		XXVI		XXVII		XXVIII		XXIV	
	WORDS	C.R.	POETRY	C.R.	WORDS	C.R.	POETRY	C.R.	TOTALS	C.R.
A	78.8	(2)	93.4	(4)	96.7	(1)	57.9	(5)	326.8	(2)
B	82.3	(1)	81.3	(6)	93.3	(2)	80.8	(1)	337.7	(1)
C	45.8	(7)	80.6	(8)	76.4	(8)	32.6	(7)	235.4	(7½)
D	65.3	(3)	48.0	(9)	71.5	(9)	31.1	(8)	215.9	(9)
E	56.6	(4)	92.6	(5)	76.8	(7)	65.6	(3)	291.6	(4)
F	34.2	(9½)	94.4	(2½)	82.4	(5)	24.4	(10)	235.4	(7½)
G	44.4	(8)	43.2	(10)	85.7	(4)	26.8	(9)	200.1	(10)
H	54.1	(5)	95.4	(1)	80.5	(6)	50.0	(6)	280.0	(5)
J	47.7	(6)	81.0	(7)	89.2	(3)	76.7	(2)	294.6	(3)
K	34.2	(9½)	94.4	(2½)	71.4	(10)	58.0	(4)	258.0	(6)

The most obvious result of these tests is that there are very wide differences in the abilities of the several subjects to attend to what is being heard and what is being seen at the same time. The order of ability to 'span' both visual and auditory presentations clearly corresponds with the order found for span when the perceptions were visual and visual-motor. This appears in several correlations between the Tables for Span and those above, (see Table of Significant Correlations). This correlating of the visual, motor and auditory factors in attention confirms what was said in the chapter on Celerity, that the experiments measure actual differences in the attentional processes, and not mere eccentricities of perception.

The primary question in this chapter is; do those who have the clearer auditory imagination give any evidence of this trait when the attention is directed to auditory perceptions. The answer is in the high correlation coefficient for Table XXIV above, and Table XII, Ideational Types, (Auditory Type.) This correlation is the highest the Auditory Type yields. Indeed there is but one other correlation with auditory type in the entire work. There is, however, no

correlation between Auditory Type and any of the other Tables for Auditory Factors.

The Visual Type has as high a correlation with the Table XXIII, above, as does the Auditory. But it must be remembered that the visualizer has a broad span, as numerous correlations throughout have shown, and it is to be expected that his results in these experiments would total high. The Visual Type, also, shows a correlation with the ability to perceive words when the attention is engaged in hearing words. Both of these aptitudes may be due to the facility with which the visualizer catches the objects of visual perception leaving other energies of the attention free to engage with other things. This the Motor Type could not do, for his sole correlation with these tables is in the ability to attend the word series when the attention was upon the words, primarily; which bears out what has been observed before, that when the words are attended with a motor reaction, as they usually are by the Motor Type, there is little attentional energy left to be occupied with aught else.

Part III. Correlations

In the present chapter we shall compare the entire list of experiments to discover what traits of attention are related. The most satisfactory way to accomplish this is by presenting the results in one great correlation table. The significant correlations in this table are presented in a smaller table in order that they may be more readily seen and studied. (See tables at end of monograph.) After considering a number of methods for the comparing the results of the experiments, Spearman's 'Footrule' for measuring correlations was adopted. The method is explained in full in the article entitled Footrule for Measuring Correlation, by C. Spearman, in the *British Journal of Psychology*, vol. ii, pp. 89–108. Briefly, the method is this: the subjects are arranged in the order of their ability for two sets of experiments; for example, the two experiments recorded with their results in Table XXI in the chapter on Motor Factors and Table IX in the chapter

on Concentration. The second series of results is compared with the first and the sum of the gains in rank for the several subjects carefully noted. Thus:

SUBJECTS	MOTOR FACTORS	CONCENTRATION	SUM OF GAINS
	Table XXI	Table IX	
A	8	10	2
B	10	6	
C	$5\frac{1}{2}$	7	$1\frac{1}{2}$
D	$5\frac{1}{2}$	3	
E	7	5	
F	3	8	5
G	4	4	
H	9	9	
J	2	2	
K	1	1	
Total			$8\frac{1}{2}$

The sum of gains in rank is denoted by Σg in the following formula.

Let the Σg to be expected on an average, for mere chance be denoted by M; this amounts to $\frac{n^2 - 1}{6}$ when n is the number of cases in each series. (For proof see page 105 of Spearman's article). Then the coefficient, say $R = 1 - \frac{\Sigma g}{M}$.

In the present experiments $n = 10$. So that $M = \frac{10^2 - 1}{6} = \frac{99}{6} = 16.5$. Then the correlation for the above tables will be

$$R = 1 - \frac{\Sigma g}{M} = 1 - \frac{8.5}{16.5} = 0.4849.$$

It must be remembered that in this (and in almost every) probability formula, any experimental result such as R has no scientific significance —except negatively—unless it be at least twice as great as its probable error; for otherwise it is almost as likely as not to be a chance coincidence. To be fairly good evidence, the R must be over three times greater than its probable error (*Ibid.*, p. 96).

In the following tables the decimal is carried only to the second place as the numbers are sufficiently far apart to make decimals of the second and third places unnecessary.

The Probable Error may be taken with sufficient nearness as being $\frac{0.43}{\sqrt{n}}$. (For proof see p. 106, *Ibid.*) The Probable Error in the following Table is:

$$\frac{0.43}{\sqrt{10}} = \frac{0.43}{3.162} = 0.136.$$

In this way we learn that our correlation in the above tables is just about large enough to be beyond reasonable suspicion of chance coincidence.

Positive correlations point out relationships which actually exist between two mental traits. The negative correlations show that the traits compared do *not* exist in conjunction, but that where we find one the other will be absent. Negative correlations are of value in corroborating, or contradicting the positive.

In the correlation formula used a large negative correlation may be changed to a positive if one of the two series being compared is inverted. Thus, if the sum of the gains in the second of the two series totals 11.5 the correlational coefficient will be 0.30. If the order of this second series were reversed the sum of the gains will be 19.55 and its R is .18.

The table of significant correlations contains all those positive correlations which are above twice the probable error. In the groups discussed below only those correlations which are above three times this probable error are considered; for, as Spearman points out, a low correlation is not trustworthy. It is important, however, if it occurs frequently. For that reason the smaller figures appear in the positive correlation table. In arranging the correlation results, below, each experiment is given and its correlations with the others. The correlational coefficient is given in parentheses and the experiment is briefly described with an abbreviated reference to the chapter and table where it is described in detail.

Span

Totals for all experiments in Span (Span IV):

Correlate (0.54) with Visual Imagination (Ideat. Types XI).

Correlate (0.57) with Ability in Attending color-class (Vis. Per. XVI).

Correlate (0.42) with Adherence to color-class (Vis. Per. XIX).

Span of Attention for Colored Objects (Span I):

Correlate (0.63) with Instinctive Selection of color-class (Vis. Per. XIV).

Correlate (0.54) with Ability in Attending color-class (Vis. Per. XVI).

Correlate (0.45) with Retention in Fringe of Attention of former objects seen (Mem. Factors).

Correlate (0.45) with Visual Imagination (Ideat. Types XI).

Span of attention for Colored Letters And Numbers (Span III):

Correlate (0.66) with Ability in Attending color-class (Vis. Per. XVI).

Correlates (0.45) with Visual Imagination (Ideat. Types XI).

Correlations between the several Experiments upon Span.

Span for colored objects (Span I) correlates with span for colored letters and figures, (Span III) and the Totals for all Span Experiments (Span IV), Correlate (63) with Span I, (42) with Span II and (0.78) with Span III;

Concentration and Inhibition

Ability to concentrate upon one class of Objects (Concent., etc. V).

Correlate (0.51) Preference for word-class (Motor Fac. XXIII).

Ability to concentrate when objects are upon Colored Backgrounds (Concent. VII).

Correlate (0.45) Ability in color-class (Vis. Per. XVII).

Ability to concentrate upon words of a certain Class (Concent. etc. IX).

Correlate (0.48) Instinctive Selection of the word-class (Motor Fac. XXI).

Ideational Types

Visual Imagination (Ideat. Types XI):

Correlate (0.57) with Ability in Attending color-class (Vis. Per. XVI).

Correlate (0.51) with Ability to Associate colors with their objects (Vis. Per. XV).

Correlate (0.42) with Instinctive Selection of the color-class (Vis. Per. XIV).

Correlate (0.48) with Span of Auditory Attention (Aud. Factors XXIV).

Auditory Imagination (Ideat. Types XII).

Correlate (0.48) with Span of Auditory Attention (Aud. Factors XXIV).

Correlate (0.42) Concentration of Attention during Fire-alarm distraction (Concent. etc. VIII).

Motor Imagination (Ideat. Types III).

Correlate (0.45) with Ability to associate colors with their objects (Vis. Per. XV).

Motor Factors, etc.

Ability in Attending the word-class (Motor Fac. XXII).

Correlate (0.51) with ability to attend Poetry read while concentrating primarily on words seen (Aud. XXVIII).

Ability in attending the word-class (Motor Fac. XXI).

Correlate (0.42) with memory experiments.

Visual Perception, etc.

Instinctive selection of color-class (Vis. Per. XIV).

Correlate (0.45) with the ability to shift the attention from one class to another (Mob. X).

Ability in Attending the color-class (Vis. Per. XVI) correlates.

(0.42) Totals for Auditory work (Aud. XXIV).

The Adherence to the color-class when the word is introduced (Vis. Per. XIX).

Correlate (0.42) with Auditory spanning of attention. (Aud. XXVI)

The Experiments upon Visual Perception and Attention correlate among themselves as follows. The ability to attend the color-class correlates (0.48) with the adherence to the color-class when the word-class is also introduced. The two similar experiments upon ability to attend the color-class correlate (0.42) with each other.

Auditory Factors in Perceptual Attention

The majority of correlations between the auditory experiments and the other experiments appear in the above groups. It remains for this group to note simply the correlations of these experiments with each other. The totals for Span in all the auditory work (Aud. XXIV) correlate (0.60) with the ability to attend to poetry read when the attention is primarily upon words seen (Aud. XXVIII), and the totals, also correlate(0.48) with the ability to attend the words seen in the same tests (Aud. XXV). It is also noteworthy that the totals for these experiments give a ranking which correlates with three out of the four of the constituent experiments.

From the above correlations it may be inferred that the observer who is broad-spanned for all classes of objects seen is of the visual type of imagination. Further, he shows a preference and an unique ability in attending to the color-class of objects. The ability to attend to a large number of colored objects is coupled with an instinctive preference for that class and is related to the faculty of carrying the impression of former observations in the fringe of attention.

The results for concentration are rather scattering. The knack of inhibiting the distraction of a colored background is related to special ability in attending to the color-class. Why this should be so is hard to understand. The ability to concentrate upon the word-class and to 'set the attention' for a certain class of words both seem to be connected with a fondness for the word-class. This may be due to a motor setting of the attention.

The Ideational Types are most instructive in the relation the Visual Imagination sustains to visual perception. There can be little doubt that the visualizer is more successful in attending to the color class than to the words. It is curious that the visualizer also does well in the auditory work. The very few correlations that the Auditory Type of Imagination makes renders its correlation with span for auditory work significant. This correlational value with auditory span is the same for the visualizer, and it must be remembered that the attention

was directed to visual as well as auditory perceptions in the auditory work. An unusual correlation is that between the ability to span visual and auditory perceptions synchronously and the ability to inhibit auditory distractions. The connection between the motor type of imagery and the ability to connect a color with the object upon which the color appeared would seem to imply a relation between visual and motor retention of objects observed.

The connection between the faculty for attending to the word-class and for attending to poetry while viewing words may point to a motor repetition of the verses which would be easier for the moteur than the visualizer. Ability in the word-class correlates with the holding of objects previously observed in the fringe of attention.

In the experiments upon Visual Perception and Attention the most significant fact is the correlation between the different experiments in this same class. It is evident that there is some underlying trait of attention which appears in these tests. It is probably an ability to attend, remember and imagine in visual terms better than in others.

The auditory-visual experiments show rather scattering correlations. Their correlations among themselves show they point in one general direction. The lower correlations show an interesting relation to span for visual work.

A comparison of the Tables of Significant Correlations and Negative Correlations is one of the best indications that the experiments actually point out typical traits in Attention and that the results are not accidental.

If the Significant Correlations were in about the same proportion as the Negative for each set of experiments, it would be apparent that the whole work is unreliable, and the results fortuitous. This is not the case. Following the correlations for Span across the Table it appears that the Positive and Negative correlations, which are significant, occur in the following proportions; 3 : 0; 2 : 6; 0 : 0; 3 : 0; 10 : 1; 0 : 7; 5 : 1; 2 : 0. Those for Visual Perception are much more striking; 8 : 0; 0 : 13; 7 : 0; 1 : 1. The negative correlations support the positive in nearly every case. The results for

Concentration and Inhibition are, as might be expected, the least reliable.

The negative correlation between concentration experiments and Span is a further support to what has been found before, that span and concentration do not seem to be related.

The many negative correlations between Span and Motor Factors show conclusively that the motor element in attending to the word-class is a feature of attention which does not go with Span.

It is interesting to note that the only positive correlation Mobility makes is with Instinctive preference for color-class and the only high negative is with Preference for the word-class.

The five negative correlations between Visual and Auditory Types and the three experiments on Motor Factors are not high enough to carry much weight but they are suggestive in showing a negative relation between Motor Factors and the Visual and the Auditory Types but not the Motor Type.

The many negative correlations between the experiments with Motor Factors and in Visual Perception show again, that the series of experiments affected attention in characteristically different ways.

In summing up the results of the Negative Correlations it may be said that they corroborate the findings of the Positive Correlations, but do not add to our information materially. A casual comparison of the two Tables shows this clearly. A more exact study brings to light the fact, noted earlier in the text, that those experiments whose results were not entirely clear (such as the work in Concentration) have a more even proportion of positive and negative correlations than do the experiments upon Span, Types, Motor and Visual Factors, etc. In the latter the proportions for the two classes of correlation are thoroughly consistent and convincing.

CONCLUSION

From the foregoing correlations and discussions the following conclusions are deduced, concerning Types of Attention,

under the conditions of experimentation described in the preceding chapters.

1. There are broad and narrow spanned types of attentional activity. The broad spanned type for visual perceptions is also broad spanned for auditory perceptions (umfang).

2. There is also a type of attention which is alert, active, under quick control; and there is a type which moves sluggishly. The former is broad spanned.

3. The ability to concentrate and inhibit does not appear in close relation with any other marked traits of attention. This ability varies in individuals but not in a manner which gives evidence of type.

4. The dexterity or suppleness in control of attention is another feature which cannot be classed as a Type.

5. The impressions which catch in the 'Fringe' of attention and later enter the 'Clearness Area,' vary characteristically with individuals. The type most susceptible to this experience is also broad spanned.

6. The Visualizer is broad spanned for both visual and auditory perceptions.

7. The 'Auditif' shows his attentional type in the ability to inhibit sound and in the breadth of span for visual and auditory impressions presented synchronously.

8. The Motor Type of ideation makes so few correlations that its evidence is largely negative. It is not broad spanned for the work given in these experiments. It is probably more efficient in concentration than the Visual.

In view of these resuls it must be acknowledged that the Attention is a function of the co-operation of many factors of the mind and it takes its character from them. The activities of the Attention will not be understood until the relation to these component and controlling factors is understood. As Goethe has well said:

> "Das Besondere unterliegt ewig dem Allgemeinen
> Das Allgemeine hat ewig sich dem Besonderen zu fügen."

56.

I.

PSYCHOLOGY AND INDUSTRIAL EFFICIENCY

PSYCHOLOGY AND INDUSTRIAL EFFICIENCY

BY

HUGO MÜNSTERBERG

BOSTON AND NEW YORK
HOUGHTON MIFFLIN COMPANY
The Riverside Press Cambridge
1913

Published February 1913

VII

CONTENTS

CONTENTS

INTRODUCTION

PSYCHOLOGY AND INDUSTRIAL EFFICIENCY

INTRODUCTION

I

APPLIED PSYCHOLOGY

OUR aim is to sketch the outlines of a new science which is to intermediate between the modern laboratory psychology and the problems of economics: the psychological experiment is systematically to be placed at the service of commerce and industry. So far we have only scattered beginnings of the new doctrine, only tentative efforts and disconnected attempts which have started, sometimes in economic, and sometimes in psychological, quarters. The time when an exact psychology of business life will be presented as a closed and perfected system lies very far distant. But the earlier the attention of wider circles is directed to its beginnings and to the importance and bearings of its tasks, the quicker and the more sound will be the development of this young science. What is most needed to-day at the beginning of the new movement are

clear, concrete illustrations which demonstrate the possibilities of the new method. In the following pages, accordingly, it will be my aim to analyze the results of experiments which have actually been carried out, experiments belonging to many different spheres of economic life. But these detached experiments ought always at least to point to a connected whole; the single experiments will, therefore, always need a general discussion of the principles as a background. In the interest of such a wider perspective we may at first enter into some preparatory questions of theory. They may serve as an introduction which is to lead us to the actual economic life and the present achievements of experimental psychology.

It is well known that the modern psychologists only slowly and very reluctantly approached the apparently natural task of rendering useful service to practical life. As long as the study of the mind was entirely dependent upon philosophical or theological speculation, no help could be expected from such endeavors to assist in the daily walks of life. But half a century has passed since the study of consciousness was switched into the tracks of exact scientific investigation. Five decades ago the psychologists began to devote themselves to the most minute description of the mental experiences and to explain the mental life

in a way which was modeled after the pattern of exact natural sciences. Their aim was no longer to speculate about the soul, but to find the psychical elements and the constant laws which control their connections. Psychology became experimental and physiological. For more than thirty years the psychologists have also had their workshops. Laboratories for experimental psychology have grown up in all civilized countries, and the new method has been applied to one group of mental traits after another. And yet we stand before the surprising fact that all the manifold results of the new science have remained book knowledge, detached from any practical interests. Only in the last ten years do we find systematic efforts to apply the experimental results of psychology to the needs of society.

It is clear that the reason for this late beginning is not an unwillingness of the last century to make theoretical knowledge serviceable to the demands of life. Every one knows, on the contrary, that the glorious advance of the natural sciences became at the same time a triumphal march of technique. Whatever was brought to light in the laboratories of the physicists and chemists, of the physiologists and pathologists, was quickly transformed into achievements of physical and chemical industry, of medicine and hygiene, of agricul-

ture and mining and transportation. No realm of the external social life remained untouched. The scientists, on the other hand, felt that the far-reaching practical effect which came from their discoveries exerted a stimulating influence on the theoretical researches themselves. The pure search for truth and knowledge was not lowered when the electrical waves were harnessed for wireless telegraphy, or the Roentgen rays were forced into the service of surgery. The knowledge of nature and the mastery of nature have always belonged together.

The persistent hesitation of the psychologists to make similar practical use of their experimental results has therefore come from different causes. The students of mental life evidently had the feeling that quiet, undisturbed research was needed for the new science of psychology in order that a certain maturity might be reached before a contact with the turmoil of practical life would be advisable. The sciences themselves cannot escape injury if their results are forced into the rush of the day before the fundamental ideas have been cleared up, the methods of investigation really tried, and an ample supply of facts collected. But this very justified reluctance becomes a real danger if it grows into an instinctive fear of coming into contact at all with practical life. To be sure,

in any single case there may be a difference of opinion as to when the right time has come and when the inner consolidation of a new science is sufficiently advanced for the technical service, but it ought to be clear that it is not wise to wait until the scientists have settled all the theoretical problems involved. True progress in every scientific field means that the problems become multiplied and that ever new questions keep coming to the surface. If the psychologists were to refrain from practical application until the theoretical results of their laboratories need no supplement, the time for applied psychology would never come. Whoever looks without prejudice on the development of modern psychology ought to acknowledge that the hesitancy which was justified in the beginning would to-day be inexcusable lack of initiative. For the sciences of the mind, too, the time has come when theory and practice must support each other. An exceedingly large mass of facts has been gathered, the methods have become refined and differentiated, and however much may still be under discussion, the ground common to all is ample enough to build upon.

Another important reason for the slowness of practical progress was probably this. When the psychologists began to work with the new experimental methods, their most immediate concern

was to get rid of mere speculation and to take hold of actual facts. Hence they regarded the natural sciences as their model, and, together with the experimental method which distinguishes scientific work, the characteristic goal of the sciences was accepted too. This scientific goal is always the attainment of general laws; and so it happened that in the first decades after the foundation of psychological laboratories the general laws of the mind absorbed the entire attention and interest of the investigators. The result of such an attitude was, that we learned to understand the working of the typical mind, but that all the individual variations were almost neglected. When the various individuals differed in their mental behavior, these differences appeared almost as disturbances which the psychologists had to eliminate in order to find the general laws which hold for every mind. The studies were accordingly confined to the general averages of mental experience, while the variations from such averages were hardly included in the scientific account. In earlier centuries, to be sure, the interest of the psychological observers had been given almost entirely to the rich manifoldness of human characters and intelligences and talents. In the new period of experimental work, this interest was taken as an indication of the unscientific fancies

of the earlier age, in which the curious and the anecdotal attracted the view. The new science which was to seek the laws was to overcome such popular curiosity. In this sign experimental psychology has conquered. The fundamental laws of the ideas and of the attention, of the memory and of the will, of the feeling and of the emotions, have been elaborated. Yet it slowly became evident that such one-sidedness, however necessary it may have been at the beginning, would make any practical application impossible. In practical life we never have to do with what is common to all human beings, even when we are to influence large masses; we have to deal with personalities whose mental life is characterized by particular traits of nationality, or race, or vocation, or sex, or age, or special interests, or other features by which they differ from the average mind which the theoretical psychologist may construct as a type. Still more frequently we have to act with reference to smaller groups or to single individuals whose mental physiognomy demands careful consideration. As long as experimental psychology remained essentially a science of the mental laws, common to all human beings, an adjustment to the practical demands of daily life could hardly come in question. With such general laws we could never have mastered the con-

crete situations of society, because we should have had to leave out of view the fact that there are gifted and ungifted, intelligent and stupid, sensitive and obtuse, quick and slow, energetic and weak individuals.

But in recent years a complete change can be traced in our science. Experiments which refer to these individual differences themselves have been carried on by means of the psychological laboratory, at first reluctantly and in tentative forms, but within the last ten years the movement has made rapid progress. To-day we have a psychology of individual variations from the point of view of the psychological laboratory.[1] This development of schemes to compare the differences between the individuals by the methods of experimental science was after all the most important advance toward the practical application of psychology. The study of the individual differences itself is not applied psychology, but it is the presupposition without which applied psychology would have remained a phantom.

II

THE DEMANDS OF PRACTICAL LIFE

WHILE in this way the progress of psychology itself and the development of the psychology of individual differences favored the growth of applied psychology, there arose at the same time an increasing demand in the midst of practical life. Especially the teachers and the physicians, later the lawyers as well, looked for help from exact psychology. The science of education and instruction had always had some contact with the science of the mind, as the pedagogues could never forget that the mental development of the child has to stand in the centre of educational thought. For a long while pedagogy was still leaning on a philosophical psychology, after that old-fashioned study of the soul had been given up in psychological quarters. At last, in the days of progressive experimental psychology, the time came when the teachers under the pressure of their new needs began to inquire how far the modern laboratory could aid them in the classroom. The pedagogical psychology of memory, of attention, of will, and of intellect was systematically worked up by men with practical

school interests. We may notice in the movement a slow but most important shifting. At first the results of theoretical psychology were simply transplanted into the pedagogical field. Experiments which were carried on in the interest of pure theoretical science were made practical use of, but their application remained a mere chance by-product. Only slowly did the pedagogical problems themselves begin to determine the experimental investigation. The methods of laboratory psychology were applied for the solving of those problems which originated in the school experience, and only when this point was reached could a truly experimental pedagogy be built on a psychological foundation. We stand in the midst of this vigorous and healthy movement, which has had a stimulating effect on theoretical psychology itself.

We find a similar situation in the sphere of the physician. He could not pass by the new science of the mind without instinctively feeling that his medical diagnosis and therapy could be furthered in many directions by the experimental method. Not only the psychiatrist and nerve specialist, but in a certain sense every physician had made use of a certain amount of psychology in his professional work. He had always had to make clear to himself the mental experiences of the patient,

to study his pain sensations and his feelings of comfort, his fears and his hopes, his perceptions and his volitions, and to a certain degree he had always tried to influence the mental life of the patient, to work on him by suggestion and to help him by stimulating his mind. But as far as a real description and explanation of such mental experiences came in question, all remained a dilettantic semi-psychology which worked with the most trivial conceptions of popular thinking. The medical men recognized the disproportion between the exactitude of their anatomical, physiological, and pathological observation and the superficiality of their self-made psychology. Thus the desire arose in their own medical circle to harmonize their psychological means of diagnosis and therapy with the schemes of modern scientific psychology. The physician who examines the sensations in a nervous disease, or the intelligence in a mental disease, or heals by suggestion or hypnotism, tries to apply the latest discoveries of the psychological laboratory. But here, too, the same development as in pedagogy can be traced. The physicians at first made use only of results which had been secured under entirely different points of view, but later the experiments were subordinated to the special medical problems. Then the physician was no longer obliged simply

to use what he happened to find among the results of the theoretical psychologist, but carried on the experiments in the service of medical problems. The independent status of experimental medical psychology could be secured only by this development.

In somewhat narrower limits the same may be said as to the problems of law. A kind of popular psychology was naturally involved whenever judges or lawyers analyzed the experience on the witness stand or discussed the motives of crime or the confessions of the criminal or the social conditions of criminality. But when every day brought new discoveries in the psychological laboratory, it seemed natural to make use of the new methods and of the new results in the interest of the courtroom. The power of observation in the witness, the exactitude of his memory, the character of his illusions and imagination, his suggestibility and his feeling, appeared in a new light in view of the experimental investigations, and the emotions and volitions of the criminal were understood with a new insight. Here, too, the last step was taken. Instead of being satisfied with experiments which the psychologist had made for his own purposes, the students of legal psychology adjusted experiments to the particular needs of the courtroom. Investigations were carried on to

determine the fidelity of testimony or to find methods for the detection of hidden thoughts and so on. Efforts toward the application of psychology have accordingly grown up in the fields of pedagogy, medicine, and jurisprudence, but as these studies naturally do not remain independent of one another, they all together form the one unified science of applied psychology.[2]

As soon as the independence of this new science was felt, it was natural that new demands and new problems should continue to originate within its own limits. There must be applied psychology wherever the investigation of mental life can be made serviceable to the tasks of civilization. Criminal law, education, medicine, certainly do not constitute the totality of civilized life. It is therefore the duty of the practical psychologist systematically to examine how far other purposes of modern society can be advanced by the new methods of experimental psychology. There is, for instance, already far-reaching agreement that the problems of artistic creation, of scientific observation, of social reform, and many similar endeavors must be acknowledged as organic parts of applied psychology. Only one group of purposes is so far surprisingly neglected in the realm of the psychological laboratory: the purposes of the economic life, the purposes of commerce and

industry, of business and the market in the widest sense of the word. The question how far applied psychology can be extended in this direction is the topic of the following discussions.

III

MEANS AND ENDS

APPLIED psychology is evidently to be classed with the technical sciences. It may be considered as psychotechnics, since we must recognize any science as technical if it teaches us to apply theoretical knowledge for the furtherance of human purposes. Like all technical sciences, applied psychology tells us what we ought to do if we want to reach certain ends; but we ought to realize at the threshold where the limits of such a technical science lie, as they are easily overlooked, with resulting confusion. We must understand that every technical science says only: you must make use of this means, if you wish to reach this or that particular end. But no technical science can decide within its limits whether the end itself is really a desirable one. The technical specialist knows how he ought to build a bridge or how he ought to pierce a tunnel, presupposing that the bridge or the tunnel is desired. But whether they are desirable or not is a question which does not concern the technical scientist, but which must be considered from

economic or political or other points of view. Everywhere the engineer must know how to reach an end, and must leave it to others to settle whether the end is in itself desirable. Often the end may be a matter of course for every reasonable being. The extreme case is presented by the applied science of medicine, where the physician subordinates all his technique to the end of curing the patient. Yet if we are consistent we must acknowledge that all his medical knowledge can prescribe to him only that he proceed in a certain way if the long life of the patient is acknowledged as a desirable end. The application of anatomy, physiology, and pathology may just as well be used for the opposite end of killing a man. Whether it is wise to work toward long life, or whether it is better to kill people, is again a problem which lies outside the sphere of the applied sciences. Ethics or social philosophy or religion have to solve these preliminary questions. The physician as such has only to deal with the means which lead toward that goal.

We must make the same discrimination in the psychotechnical field. The psychologist may point out the methods by which an involuntary confession can be secured from a defendant, but whether it is justifiable to extort involuntary confessions is a problem which does not concern

the psychologist. The lawyers or the legislators must decide as to the right or wrong, the legality or illegality, of forcing a man to show his hidden ideas. If such an end is desirable, the psychotechnical student can determine the right means, and that is the limit of his office. We ought to keep in mind that the same holds true for the application of psychology in economic life. Economic psychotechnics may serve certain ends of commerce and industry, but whether these ends are the best ones is not a care with which the psychologist has to be burdened. For instance, the end may be the selection of the most efficient laborers for particular industries. The psychologist may develop methods in his laboratory by which this purpose can be fulfilled. But if some mills prefer another goal, — for instance, to have not the most efficient but the cheapest possible laborers, — entirely different means for the selection are necessary. The psychologist is, therefore, not entangled in the economic discussions of the day; it is not his concern to decide whether the policy of the trusts or the policy of the trade-unions or any other policy for the selection of laborers is the ideal one. He is confined to the statement: if you wish this end, then you must proceed in this way; but it is left to you to express your preference among the ends. Applied psychology can,

therefore, speak the language of an exact science in its own field, independent of economic opinions and debatable partisan interests. This is a necessary limitation, but in this limitation lies the strength of the new science. The psychologist may show how a special commodity can be advertised; but whether from a social point of view it is desirable to reinforce the sale of these goods is no problem for psychotechnics. If a sociologist insists that it would be better if not so many useless goods were bought, and that the aim ought rather to be to protect the buyer than to help the seller, the psychologist would not object. His interest would only be to find the right psychological means to lead to this other social end. He is partisan neither of the salesman nor of the customer, neither of the capitalist nor of the laborer, he is neither Socialist nor anti-Socialist, neither high-tariff man nor free-trader. Here, too, of course, there are certain goals which are acknowledged on all sides, and which therefore hardly need any discussion, just as in the case of the physician, where the prolongation of life is practically acknowledged as a desirable end by every one. But everywhere where the aim is not perfectly a matter of course, the psychotechnical specialist fulfills his task only when he is satisfied with demonstrating that certain psychical means serve a cer-

tain end, and that they ought to be applied as soon as that end is accepted.

The whole system of psychotechnical knowledge might be subdivided under either of the two aspects. Either we might start from the various mental processes and ask for what end each mental factor can be practically useful and important, or we can begin with studying what significant ends are acknowledged in our society and then we can seek the various psychological facts which are needed as means for the realization of these ends. The first way offers many conveniences. There we should begin with the mental states of attention, memory, feeling, and so on, and should study how the psychological knowledge of every one of these mental states can render service in many different practical fields. The attention, for instance, is important in the classroom when the teacher tries to secure the attention of the pupils, but the judge expects the same attention from the jurymen in the courtroom, the artist seeks to stir up the attention of the spectator, the advertiser demands the attention of the newspaper readers. Whoever studies the characteristics of the mental process of attention may then be able to indicate how in every one of these unlike cases the attention can be stimulated and retained. Nevertheless the opposite way which starts from

the tasks to be fulfilled seems more helpful and more fundamentally significant. The question, then, is what mental processes become important for the tasks of education, what for the purposes of the courtroom, what for the hospital, what for the church, what for politics, and so on.

As this whole essay is to be devoted exclusively to the economic problems, we are obliged to choose the second way; that is, to arrange applied psychology with reference to its chief ends and not with reference to the various means. But the same question comes up in the further subdivision of the material. In the field of economic psychology, too, we might ask how far the study of attention, or of perception, or of feeling, or of will, or of memory, and so on, can be useful for the purposes of the business man. Or here, too, we might begin with the consideration of the various ends and purposes. The ends of commerce are different from those of industry, those of publishing different from those of transportation, those of agriculture different from those of mining; or, in the field of commerce, the purposes of the retailer are different from those of the wholesale merchant. There can be no limit to such subdivisions; each particular industry has its own aims, and in the same industry a large variety of tasks are united. We should accordingly be led to an ample classi-

fication of special economic ends with pigeonholes for every possible kind of business and of labor. The psychologist would have to find for every one of these ends the right mental means. This would be the ideal system of economic psychology.

But we are still endlessly far from such a perfect system. Modern educational psychology and medical psychology have reached a stage at which an effort for such a complete system might be realized, but economic psychology is still at too early a stage of development. It would be entirely artificial to-day to aim at such ideal completeness. If we were to construct such a complete system of questions, we should have no answers. In the present stage nothing can be seriously proposed but the selection of a few central purposes which occur in every department of business life, and a study of the means to reach these special ends by the discussion of some typical cases which may clearly illustrate the methods involved.

From this point of view we select three chief purposes of business life, purposes which are important in commerce and industry and every economic endeavor. We ask how we can find the men whose mental qualities make them best fitted for the work which they have to do; secondly, under what psychological conditions we can secure the greatest and most satisfactory output

of work from every man; and finally, how we can produce most completely the influences on human minds which are desired in the interest of business. In other words, we ask how to find the best possible man, how to produce the best possible work, and how to secure the best possible effects.

PART I

THE BEST POSSIBLE MAN

PART I

THE BEST POSSIBLE MAN

IV

VOCATION AND FITNESS

INSTEAD of lingering over theoretical discussions, we will move straight on toward our first practical problem. The economic task, with reference to which we want to demonstrate the new psychotechnic method, is the selection of those personalities which by their mental qualities are especially fit for a particular kind of economic work. This problem is especially useful to show what the new method can do and what it cannot do. Whether the method is sufficiently developed to secure full results to-day, or whether they will come to-morrow, is unimportant. It is clear that the success of to-morrow is to be hoped for, only if understanding and interest in the problem is already alive to-day.

When we inquire into the qualities of men, we use the word here in its widest meaning. It covers, on the one side, the mental dispositions which may still be quite undeveloped and which may unfold

only under the influence of special conditions in the surroundings; but, on the other side, it covers the habitual traits of the personality, the features of the individual temperament and character, of the intelligence and of the ability, of the collected knowledge and of the acquired experience. All variations of will and feeling, of perception and thought, of attention and emotion, of memory and imagination, are included here. From a purely psychological standpoint, quite incomparable contents and functions and dispositions of the personality are thus thrown together, but in practical life we are accustomed to proceed after this fashion: If a man applies for a position, he is considered with regard to the totality of his qualities, and at first nobody cares whether the particular feature is inherited or acquired, whether it is an individual chance variation or whether it is common to a larger group, perhaps to all members of a certain nationality or race. We simply start from the clear fact that the personalities which enter into the world of affairs present an unlimited manifoldness of talents and abilities and functions of the mind. From this manifoldness, it necessarily follows that some are more, some less, fit for the particular economic task. In view of the far-reaching division of labor in our modern economic life, it is impossible to avoid the ques-

tion how we can select the fit personalities and reject the unfit ones.

How has modern society prepared itself to settle this social demand? In case that certain knowledge is indispensable for the work or that technical abilities must have been acquired, the vocation is surrounded by examinations. This is true of the lower as well as of the higher activities. The direct examination is everywhere supplemented by testimonials covering the previous achievements, by certificates referring to the previous education, and in frequent cases by the endeavor to gain a personal impression from the applicant. But if we take all this together, the total result remains a social machinery by which perhaps the elimination of the entirely unfit can be secured. But no one could speak of a really satisfactory adaptation of the manifold personalities to the economic vocational tasks. All those examinations and tests and certificates refer essentially to what can be learned from without, and not to the true qualities of the mind and the deeper traits. The so-called impressions, too, are determined by the most secondary and external factors. Society relies instinctively on the hope that the natural wishes and interests will push every one to the place for which his dispositions, talents, and psychophysical gifts prepare him.

In reality this confidence is entirely unfounded. A threefold difficulty exists. In the first place, young people know very little about themselves and their abilities. When the day comes on which they discover their real strong points and their weaknesses, it is often too late. They have usually been drawn into the current of a particular vocation, and have given too much energy to the preparation for a specific achievement to change the whole life-plan once more. The entire scheme of education gives to the individual little chance to find himself. A mere interest for one or another subject in school is influenced by many accidental circumstances, by the personality of the teacher or the methods of instruction, by suggestions of the surroundings and by home traditions, and accordingly even such a preference gives rather a slight final indication of the individual mental qualities. Moreover, such mere inclinations and interests cannot determine the true psychological fitness for a vocation. To choose a crude illustration, a boy may think with passion of the vocation of a sailor, and yet may be entirely unfit for it, because his mind lacks the ability to discriminate red and green. He himself may never have discovered that he is color-blind, but when he is ready to turn to the sailor's calling, the examination of his color-sensitiveness which is demanded

may have shown the disturbing mental deficiency. Similar defects may exist in a boy's attention or memory, judgment or feeling, thought or imagination, suggestibility or emotion, and they may remain just as undiscovered as the defect of color-blindness, which is characteristic of four per cent of the male population. All such deficiencies may be dangerous in particular callings. But while the vocation of the ship officer is fortunately protected nowadays by such a special psychological examination, most other vocations are unguarded against the entrance of the mentally unfit individuals.

As the boys and girls grow up without recognizing their psychical weaknesses, the exceptional strength of one or another mental function too often remains unnoticed by them as well. They may find out when they are favored with a special talent for art or music or scholarship, but they hardly ever know that their attention, or their memory, or their will, or their intellectual apprehension, or their sensory perceptions, are unusually developed in a particular direction; yet such an exceptional mental disposition might be the cause of special success in certain vocations. But we may abstract from the extremes of abnormal deficiency and abnormal overdevelopment in particular functions. Between them we find the

broad region of the average minds with their numberless variations, and these variations are usually quite unknown to their possessors. It is often surprising to see how the most manifest differences of psychical organization remain unnoticed by the individuals themselves. Men with a pronounced visual type of memory and men with a marked acoustical type may live together without the slightest idea that their contents of consciousness are fundamentally different from each other. Neither the children nor their parents nor their teachers burden themselves with the careful analysis of such actual mental qualities when the choice of a vocation is before them. They know that a boy who is completely unmusical must not become a musician, and that the child who cannot draw at all must not become a painter, just as on physical grounds a boy with very weak muscles is not fit to become a blacksmith. But as soon as the subtler differentiation is needed, the judgment of all concerned seems helpless and the psychical characteristics remain disregarded.

A further reason for the lack of adaptation, and surely a most important one, lies in the fact that the individual usually knows only the most external conditions of the vocations from which he chooses. The most essential requisite for a truly perfect adaptation, namely, a real analysis of the

vocational demands with reference to the desirable personal qualities, is so far not in existence. The young people generally see some superficial traits of the careers which seem to stand open, and, besides, perhaps they notice the great rewards of the most successful. The inner labor, the inner values, and the inner difficulties and frictions are too often unknown to those who decide for a vocation, and they are unable to correlate those essential factors of the life-calling with all that nature by inheritance, and society by surroundings and training, have planted and developed in their minds.

In addition to this ignorance as to one's own mental disposition and to the lack of understanding of the true mental requirements of the various social tasks comes finally the abundance of trivial chance influences which become decisive in the choice of a vocation. Vocation and marriage are the two most consequential decisions in life. In the selection of a husband or wife, too, the decision is very frequently made dependent upon the most superficial and trivial motives. Yet the social philosopher may content himself with the belief that even in the fugitive love desire a deeper instinct of nature is expressed, which may at least serve the biological tasks of married life. In the choice of a vocation, even such a belief in a bio-

logical instinct is impossible. The choice of a vocation, determined by fugitive whims and chance fancies, by mere imitation, by a hope for quick earnings, by irresponsible recommendation, or by mere laziness, has no internal reason or excuse. Illusory ideas as to the prospects of a career, moreover, often falsify the whole vista; and if we consider all this, we can hardly be surprised that our total result is in many respects hardly better than if everything were left entirely to accident. Even on the height of a mental training to the end of adolescence, we see how the college graduates are too often led by accidental motives to the decision whether they shall become lawyers or physicians or business men, but this superficiality of choice of course appears much more strongly where the lifework is to be built upon the basis of a mere elementary or high school education.

The final result corresponds exactly to these conditions. Everywhere, in all countries and in all vocations, but especially in the economic careers, we hear the complaint that there is lack of really good men. Everywhere places are waiting for the right man, while at the same time we find everywhere an oversupply of mediocre aspirants. This, however, does not in the least imply that there really are not enough personalities who might be perfectly fit even for the highest demands of

the vocations; it means only that as a matter of course the result in the filling of positions cannot be satisfactory, if the placing of the individuals is carried on without serious regard for the personal mental qualities. The complaint that there is lack of fit human material would probably never entirely disappear, as with a better adjustment of the material, the demands would steadily increase; but it could at least be predicted with high probability that this lack of really fit material would not be felt so keenly everywhere if the really decisive factor for the adjustment of personality and vocation, namely, the dispositions of the mind, were not so carelessly ignored.

Society, to be sure, has a convenient means of correction. The individual tries, and when he is doing his work too badly, he loses his job, he is pushed out from the career which he has chosen, with the great probability that he will be crushed by the wheels of social life. It is a rare occurrence for the man who is a failure in his chosen vocation, and who has been thrown out of it, to happen to come into the career in which he can make a success. Social statistics show with an appalling clearness what a burden and what a danger to the social body is growing from the masses of those who do not succeed and who by their lack of success become discouraged and embittered. The

social psychologist cannot resist the conviction that every single one could have found a place in which he could have achieved something of value for the commonwealth. The laborer, who in spite of his best efforts shows himself useless and clumsy before one machine, might perhaps have done satisfactory work in the next mill where the machines demand another type of mental reaction. His psychical rhythm and his inner functions would be able to adjust themselves to the requirements of the one kind of labor and not to those of the other. Truly the whole social body has had to pay a heavy penalty for not making even the faintest effort to settle systematically the fundamental problem of vocational choice, the problem of the psychical adaptation of the individuality. An improvement would lie equally in the interest of those who seek positions and those who have positions to offer. The employers can hope that in all departments better work will be done as soon as better adapted individuals can be obtained; and, on the other hand, those who are anxious to make their working energies effective may expect that the careful selection of individual mental characters for the various tasks of the world will insure not only greater success and gain, but above all greater joy in the work, deeper satisfaction, and more harmonious unfolding of the personality.

V

SCIENTIFIC VOCATIONAL GUIDANCE

OBSERVATIONS of this kind, which refer to the borderland region between psychology and social politics, are valid for all modern nations. Yet it is hardly a chance that the first efforts toward a systematic overcoming of some of these difficulties have been made with us in America. The barriers between the classes lie lower; here the choice of a vocation is less determined by tradition; and it belongs to the creed of political democracy that just as everybody can be called to the highest elective offices, so everybody ought to be fit for any vocation in any sphere of life. The wandering from calling to calling is more frequent in America than anywhere else. To be sure, this has the advantage that a failure in one vocation does not bring with it such a serious injury as in Europe, but it contributes much to the greater danger that any one may jump recklessly and without preparation into any vocational stream.

It is fresh in every one's mind how during the last decade the economic conscience of the whole American nation became aroused. Up to the end

of the last century the people had lived with the secure feeling of possessing a country with inexhaustible treasures. The last few years brought the reaction, and it became increasingly clear how irresponsible the national attitude had been, how the richness of the forests and the mines and the rivers had been recklessly squandered without any thought of the future. Conservation of the national possessions suddenly became the battle-cry, and this turned the eye also to that limitless waste of human material, a waste going on everywhere in the world, but nowhere more widely than in the United States. The feeling grew that no waste of valuable possessions is so reckless as that which results from the distributing of living force by chance methods instead of examining carefully how work and workmen can fit one another. While this was the emotional background, two significant social movements originated in our midst. The two movements were entirely independent of each other, but from two different starting-points they worked in one respect toward the same goal. They are social and economic movements, neither of which at first had anything directly to do with psychological questions; but both led to a point where the psychological turn of the problem seemed unavoidable. Here begins the obligation of the psy-

chologist, and the possibility of fulfilling this obligation will be the topic of our discussion concerning the selection of the best man.

These two American movements which we have in mind are the effort to furnish to pupils leaving the school guidance in their choice of a vocation, and the nowadays still better known movement toward scientific management in commerce and industry. The movement toward vocational guidance is externally still rather modest and confined to very narrow circles, but it is rapidly spreading and is not without significant achievements. It started in Boston. There the late Mr. Parsons once called a meeting of all the boys of his neighborhood who were to leave the elementary schools at the end of the year. He wanted to consider with them whether they had reasonable plans for their future. At the well-attended meeting it became clear that the boys knew little concerning what they had to expect in practical life, and Parsons was able to give them, especially in individual discussions, much helpful information. They knew too little of the characteristic features of the vocations to which they wanted to devote themselves, and they had given hardly any attention to the question whether they had the necessary qualifications for the special work. From this germ grew a little office which was

opened in 1908, in which all Boston boys and girls at the time when they left school were to receive individual suggestions with reference to the most reasonable and best adjusted selection of a calling. There is hardly any doubt that the remarkable success of this modest beginning was dependent upon the admirable personality of the late organizer, who recognized the individual features with unusual tact and acumen. But he himself had no doubt that such a merely impressionistic method could not satisfy the demands. He saw that a threefold advance would become necessary. First, it was essential to analyze the objective relations of the many hundred kinds of accessible vocations. Their economic, hygienic, technical, and social elements ought to be examined so that every boy and girl could receive reliable information as to the demands of the vocation and as to the prospects and opportunities in it. Secondly, it would become essential to interest the schools in all these complex questions of vocational choice, so that, by observation of individual tendencies and abilities of the pupils, the teachers might furnish preparatory material for the work of the institute for vocational guidance. Thirdly, — and this is for us the most important point, — he saw that the methods had to be elaborated in such a way that the personal traits and disposi-

tions might be discovered with much greater exactitude and with much richer detail than was possible through what a mere call on the vocational counselor could unveil.[3]

It is well known how this Boston bureau has stimulated a number of American cities to come forward with similar beginnings. The pedagogical circles have been especially aroused by the movement, municipal and philanthropic boards have at least approached this group of problems, two important conferences for vocational guidance have met in New York, and at various places the question has been discussed whether or not a vocational counselor might be attached to the schools in a position similar to that of the school physician. The chief progress has been made in the direction of collecting reliable data with reference to the economic and hygienic conditions of the various vocations, the demand and supply and the scale of wages. In short, everything connected with the externalities of the vocations has been carefully analyzed, and sufficient reliable material has been gained, at least regarding certain local conditions. In the place of individual advice, we have thus to a certain degree obtained general economic investigations from which each can gather what he needs. It seems that sometimes the danger of letting such offices degenerate

into mere agencies for employment has not been avoided, but that is one of the perils of the first development. The mother institute in Boston, too, under its new direction emphasizes more the economic and hygienic side, and has set its centre of gravity in a systematic effort to propagate understanding of the problems of vocational guidance and to train professional vocational counselors in systematic courses, who are then to carry the interest over the land.[4]

The real psychological analysis with which the movement began has, therefore, been somewhat pushed aside for a while, and the officers of those institutes declare frankly that they want to return to the mental problem only after professional psychologists have sufficiently worked out the specific methods for its mastery. Most counselors seem to feel instinctively that the core of the whole matter lies in the psychological examination, but they all agree that for this they must wait until the psychological laboratories can furnish them with really reliable means and schemes. Certainly it is very important, for instance, that boys with weak lungs be kept away from such industrial vocations as have been shown by the statistics to be dangerous for the lungs, or that the onrush to vocations be stopped where the statistics allow it to be foreseen that there will soon be an over-

supply of workers. But, after all, it remains much more decisive for the welfare of the community, and for the future life happiness of those who leave the school, that every one turn to those forms of work to which his psychological traits are adjusted, or at least that he be kept away from those in which his mental qualities and dispositions would make a truly successful advance improbable.

The problem accordingly has been handed over from the vocational counselors to the experimental psychologists, and it is certainly in the spirit of the modern tendency toward applied psychology that the psychological laboratories undertake the investigation and withdraw it from the dilettantic discussion of amateur psychologists or the mere impressionism of the school-teachers. Even those early beginnings indicate clearly that the goal can be reached only through exact, scientific, experimental research, and that the mere naïve methods — for instance, the filling-out of questionnaires which may be quite useful in the first approach — cannot be sufficient for a real, persistent furtherance of economic life and of the masses who seek their vocations. In order to gain an analysis of the individual, Parsons made every applicant answer in writing a long series of questions which referred to his habits and his

emotions, his inclinations and his expectations, his traits and his experiences. The psychologist, however, can hardly be in doubt that just the mental qualities which ought to be most important for the vocational counselor can scarcely be found out by such methods. We have emphasized before that the ordinary individual knows very little of his own mental functions: on the whole, he knows them as little as he knows the muscles which he uses when he talks or walks. Among his questions Parsons included such ones as: "Are your manners quiet, noisy, boisterous, deferential, or self-assertive? Are you thoughtful of the comfort of others? Do you smile naturally and easily, or is your face ordinarily expressionless? Are you frank, kindly, cordial, respectful, courteous in word and actions? Do you look people frankly in the eye? Are your inflections natural, courteous, modest, musical, or aggressive, conceited, pessimistic, repellent? What are your powers of attention, observation, memory, reason, imagination, inventiveness, thoughtfulness, receptiveness, quickness, analytical power, constructiveness, breadth, grasp? Can you manage people well? Do you know a fine picture when you see it? Is your will weak, yielding, vacillating, or firm, strong, stubborn? Do you like to be with people and do they like to be with you?" — and

so on. It is clear that the replies to questions of this kind can be of psychological value only when the questioner knows beforehand the mind of the youth, and can accordingly judge with what degree of understanding, sincerity, and ability the circular blanks have been filled out. But as the questions are put for the very purpose of revealing the personality, the entire effort tends to move in a circle.

To break this circle, it indeed becomes necessary to emancipate one's self from the method of ordinary self-observation and to replace it by objective experiment in the psychological laboratory. Experimentation in such a laboratory stands in no contrast to the method of introspection. A contrast does exist between self-observation and observation on children or patients or primitive peoples or animals. In their case the psychologist observes his material from without. But in the case of the typical laboratory experiment, everything is ultimately based on self-observation; only we have to do with the self-observation under exact conditions which the experimenter is able to control and to vary at will. Even Parsons sometimes turned to little experimental inquiries in which he simplified some well-known methods of the laboratory in order to secure with the most elementary means a cer-

tain objective foundation for his mental analysis. For instance, he sometimes examined the memory by reading to the boys graded sentences containing from ten to fifty words and having them repeat what they remembered, or he measured with a watch the rapidity of reading and writing, or he determined the sensitiveness for the discrimination of differences by asking them to make a point with a pencil in the centres of circles of various sizes. But if such experimental schemes, even of the simplest form, are in question, it seems a matter of course that the plan ought to be prescribed by real scientists who specialize in the psychological field. The psychologist, for instance, surely cannot agree to a method which measures the memory by such a method of having spoken sentences repeated and the quality of the memory faculty naïvely graded according to the results. He knows too well that there are many different kinds of memory, and would always determine first which type of memory functions is to be examined if memory achievements are needed for a particular calling.

But even with a more exact method of experimenting, such a procedure would not be sufficient to solve the true problem. A second step would still be necessary: namely, the adaptation of the experimental result to the special psychological

requirements of the economic activity; and this again presupposes an independent psychological analysis. Most of the previous efforts have suffered from the carelessness with which this second step was ignored, and the special mental requirements were treated as a matter of course upon which any layman could judge. In reality they need the most careful psychological analysis, and only if this is carried out with the means of scientific psychology, can a study of the abilities of the individual become serviceable to the demands of the market. Such a psychological disentangling of the requirements of the callings, in the interest of guidance, is attempted in the material which the various vocational institutes have prepared, but it seldom goes beyond commonplaces. We read there, for instance,[5] for the confectioner: "Boys in this industry must be clean, quick, and strong. The most important qualities desired are neatness and adaptability to routine"; or for the future baker, the boy "ought to know how to conduct himself and to meet the public"; or for the future architectural designer, "he must have creative ability, artistic feeling, and power to sketch"; or for the dressmaker, she "should have good eyesight and good sense of color, and an ability to use her hands readily; she should be able to apply herself steadily and be

fairly quick in her movements; neatness of person is also essential"; or for the stenographer, she must be "possessed of intelligence, good judgment, and common sense; must have good eyesight, good hearing, and a good memory; must have quick perception, and be able to concentrate her attention completely on any matter in hand." It is evident that all this is extremely far from any psychological analysis in the terms of science. All taken together, we may, therefore, say that in the movement for vocational guidance practically nothing has been done to make modern experimental psychology serviceable to the new task. But on the one side, it has shown that this work of the experimental psychologist is the next step necessary. On the other side, it has become evident that in the vocation bureaus appropriate social agencies are existing which are ready to take up the results of such work, and to apply them for the good of the American youth and of commerce and industry, as soon as the experimental psychologist has developed the significant methods.

VI

SCIENTIFIC MANAGEMENT

BEFORE we discuss some cases of such experimental investigations, we may glance at that other American movement, the well-known systematic effort toward scientific management which has often been interpreted in an expansive literature.[6] Enthusiastic followers have declared it to be the greatest advance in industry since the introduction of the mill system and of machinery. Opponents have hastily denounced it as a mistake, and have insisted that it proved a failure in the factories in which it has been introduced. A sober examination of the facts soon demonstrates that the truth lies in the middle. Those followers of Frederick W. Taylor who have made almost a religion out of his ideas have certainly often exaggerated the practical applicability of the new theories, and their actual reforms in the mills have not seldom shown that the system is still too topheavy; that is, there are too many higher employees necessary in order to keep the works running on principles of scientific management. On the other hand, the opposition which comes from certain quarters, — for instance,

from some trade-unions, — may be disregarded, as it is not directed against the claim that the efficiency can be heightened, but only against some social features of the scheme, such as the resulting temporary reduction of the number of workmen. But nobody can deny that this revolutionary movement has introduced most valuable suggestions which the industrial world cannot afford to ignore, and that as soon as exaggerations are avoided and experience has created a broader foundation, the principles of the new theory will prove of lasting value. We shall have to discuss, at a later point, various special features of the system, especially the highly interesting motion study. Here we have to deal only with those tendencies of the movement and with those interests which point toward our present problem, the mental analysis of the individual employees in order to avoid misfits.

The approach to this problem, indeed, seems unavoidable for the students of scientific management, as its goal is an organization of economic work by which the waste of energy will be avoided and the greatest increase in the efficiency of the industrial enterprise will be reached. The recognition that this can never be effected by a mere excessive driving of the workingmen belongs to its very presuppositions. The illusory means of

prolongation of the working-time and similar devices by which the situation of the individual deteriorates would be out of the question; on the contrary, the heightening of the individual's joy in the work and of the personal satisfaction in one's total life development belongs among the most important indirect agencies of the new scheme. This end is reached by many characteristic changes in the division of labor; also by a new division between supervisors and workers, by transformations of the work itself and of the tools and vehicles. But as a by-product of these efforts the demand necessarily arose for means by which the fit individuals could be found for special kinds of labor. The more scientific management introduced changes by which the individual achievement often had to become rather complicated and difficult, the more it became necessary to study the skill and the endurance and the intelligence of the individual laborers in order to entrust these new difficult tasks only to the most appropriate men in the factories and mills. The problem of individual selection accordingly forced itself on the new efficiency engineers, and they naturally recognized that the really essential traits and dispositions were the mental ones. In the most progressive books of the new movement, this need of emphasizing the selection of workers with refer-

ence to their mental equipment comes to clear expression.

Yet this is very far from a real application of scientific psychology to the problem at hand. Wherever the question of the selection of the fit men after psychological principles is mentioned in the literature of this movement, the language becomes vague, and the same men, who use the newest scientific knowledge whenever physics or mathematics or physiology or chemistry are involved, make hardly any attempts to introduce the results of science when psychology is in question. The clearest insight into the general situation may be found in the most recent books by Emerson. He says frankly: "It is psychology, not soil or climate, that enables a man to raise five times as many potatoes per acre as the average in his own state";[7] or: "In selecting human assistants such superficialities as education, as physical strength, even antecedent morality, are not as important as the inner attitudes, proclivities, character, which after all determine the man or woman."[8] He also fully recognizes the necessity of securing as early as possible the psychological essentials. He says: "The type for the great newspaper is set up by linotype operators. Apprenticeship is rigorously limited. Some operators can never get beyond the 2500-em class,

others with no more personal effort can set 5000 ems. Do the employers test out applicants for apprenticeship so as to be sure to secure boys who will develop into the 5000-em class? They do not: they select applicants for any near reason except the fundamental important one of innate fitness."[9] But all this points only to the existence of the problem, and in reality gives not even a hint for its solution. The theorists of scientific management seem to think that the most subtle methods are indispensable for physical measurements, but for psychological inquiry nothing but a kind of intuition is necessary. Emerson tells how, for instance, "The competent specialist who has supplemented natural gifts and good judgment by analysis and synthesis can perceive attitudes and proclivities even in the very young, much more readily in those semi-matured, and can with almost infallible certainty point out, not only what work can be undertaken with fair hope of success, but also what slight modification or addition and diminution will more than double the personal power."[10] The true psychological specialists surely ought to decline this flattering confidence. Far from the "almost infallible certainty," they can hardly expect even a moderate amount of success in such directions so long as specific methods have not been elaborated, and so long as no way has

been shown to make experimental measurements by which such mere guesswork can be replaced by scientific investigation.

The only modest effort to try a step in this direction toward the psychological laboratory is recorded by Taylor,[11] who tells of Mr. S. E. Thompson's work in a bicycle ball factory, where a hundred and twenty girls were inspecting the balls. They had to place a row of small polished steel balls on the back of the left hand and while they were rolled over and over in the crease between two of the fingers placed together, they were minutely examined in a strong light and the defective balls were picked out with the aid of a magnet held in the right hand. The work required the closest attention and concentration. The girls were working ten and a half hours a day. Mr. Thompson soon recognized that the quality most needed, beside endurance and industry, was a quick power of perception accompanied by quick responsive action. He knew that the psychological laboratory has developed methods for a very exact measurement of the time needed to react on an impression with the quickest possible movement; it is called the reaction-time, and is usually measured in thousandths of a second. He therefore considered it advisable to measure the reaction-time of the girls, and to eliminate from

service all those who showed a relatively long time between the stimulus and reaction. This involved laying off many of the most intelligent, hardest-working, and most trustworthy girls. Yet the effect was the possibility of shortening the hours and of reducing more and more the number of workers, with the final outcome that thirty-five girls did the work formerly done by a hundred and twenty, and that the accuracy of the work at the higher speed was two thirds greater than at the former slow speed. This allowed almost a doubling of the wages of the girls in spite of their shorter working-day, and at the same time a considerable reduction in the cost of the work for the factory. This excursion of an efficiency engineer into the psychological laboratory remained, however, an entirely exceptional case. Moreover, such a reaction-time measurement did not demand any special development of new methods or any particular mental analysis, and this exception thus confirms the rule that the followers of scientific management principles have recognized the need of psychological inquiries, but have not done anything worth mentioning to apply the results of really scientific psychology. Hence the situation is the same as in the field of vocational guidance. In both cases a vague longing for psychological analysis and psychological measure-

ment, but in both cases so far everything has remained on the level of helpless psychological dilettantism. It stands in striking contrast with the scientific seriousness with which the economic questions are taken up in the field of vocational guidance and the physical questions in the field of scientific management. It is, therefore, evidently the duty of the experimental psychologists themselves to examine the ground from the point of view of the psychological laboratory.

VII

THE METHODS OF EXPERIMENTAL PSYCHOLOGY

WE now see clearly the psychotechnical problem. We have to analyze definite economic tasks with reference to the mental qualities which are necessary or desirable for them, and we have to find methods by which these mental qualities can be tested. We must, indeed, insist on it that the interests of commerce and industry can be helped only when both sides, the vocational demands and the personal function, are examined with equal scientific thoroughness. One aspect alone is unsatisfactory. It would of course be possible to confine the examination to the individual mental traits, and then theoretically to determine for which economic tasks the presence of these qualities would be useful and for which tasks their absence or their deficiency would be fatal. Common sense may be sufficient to lead us a few steps in that direction. For instance, if we find by psychological examination that an individual is color-blind for red and green sensations, we may at once conclude, without any real psychological analysis of the vocations, that he would be unfit

for the railroad service or the naval service, in which red and green signals are of importance. We may also decide at once that such a boy would be useless for all artistic work in which the nuances of colors are of consequence, or as a laborer in certain departments of a dyeing establishment, and that such a color-blind girl would not do at a dressmaker's or in a millinery store. But if we come to the question whether such a color-blind individual may enter into the business of gardening, in spite of the inability to distinguish the strawberries in the bed or the red flowers among the green leaves, the first necessity, after all, would be to find out how far the particular demands of this vocation make the ability to discriminate color a prerequisite, and how far psychical substitutions, such as a recognition of the forms and of differences in the light intensity, may be sufficient for the practical task. Moreover, where not merely such mental defects, but more subtly shaded variations within normal limits are involved, it would be superficial, if only the mental states were examined and not at the same time the mental requirements of the vocations themselves. The vocation should rather remain the starting-point. We must at first find out what demands on the mental system are made by it, and we must grade these demands in order to recog-

nize the more or less important ones, and, especially for the important ones, we must then seek exact standards with experimental methods.

Such an experimental investigation may proceed according to either of two different principles. One way is to take the mental process which is demanded by the industrial work as an undivided whole. In this case we have to construct experimental conditions under which this total activity can be performed in a gradual, measurable way. The psychical part of the vocational work thus becomes schematized, and is simply rendered experimentally on a reduced scale. The other way is to resolve the mental process into its components and to test every single elementary function in its isolated form. In this latter case the examination has the advantage of having at its disposal all the familiar methods of experimental psychology, while in the first case for every special vocational situation perfectly new experimental tests must be devised.

Whether the one or the other method is to be preferred must depend upon the nature of the particular commercial or industrial calling, and accordingly presupposes a careful analysis of the special economical processes. It is, indeed, easy to recognize that in certain industrial activities a series of psychical functions is in question which

all lie side by side and which do not fuse into one united total process, however much they may influence one another. But for many industrial tasks just this unity is the essential condition. The testing of the mental elements would be in such cases as insufficient as if we were to test a machine with reference to its parts only and not with reference to its total united performance. Even in this latter case this unified function does not represent the total personality: it is always merely a segment of the whole mental life. We may examine with psychological methods, for instance, the fitness of an employee for a technical vocation and may test the particular complex unified combination of attention, imagination and intelligence, will and memory, which is essential for that special kind of labor. We may be able to reconstruct the conditions so completely that we would feel justified in predicting whether the individual can fulfill that technical task or not; and yet we may disregard entirely the question whether that man is honest or dishonest, whether he is pacific or quarrelsome; in short, whether his mental disposition makes him a desirable member of that industrial concern under other aspects.

We best recognize the significance of these various methods by selecting a few concrete cases as illustrations and analzying them in detail. But a

word of warning may be given beforehand so as to avoid misunderstandings. These examples do not stand here as reports of completed investigations, the results of which ought to be accepted as conclusive parts of the new psychotechnical science; they are not presented as if the results were to be recommended like a well-tested machine for practical purposes. Such really completed investigations do not as yet exist in this field. All that can be offered is modest pioneer work, and just these inquiries into the mental qualities and their relations to the industrial vocations have attracted my attention only very recently, and therefore certainly still demand long continuations of the experiments in every direction. But we may hope for satisfactory results the earlier, the more coöperators are entering the field, and the more such researches are started in other places and in other institutions. I therefore offer these early reports at the first stage of my research merely as stimulations, so as to demonstrate the possibilities.

As an illustration of the method of examining the mental process as a whole, I propose to discuss the case of the motormen in the electric railways. As an illustration of the other type, namely, of analyzing the activity and testing the elementary functions, I shall discuss the case of the employ-

ees in the telephone service. I select these two functions, as both play a practically important rôle in the technique of modern economic life and as in both occupations very large numbers of individuals are engaged in the work.

VIII

EXPERIMENTS IN THE INTEREST OF ELECTRIC RAILWAY SERVICE

THE problem of securing fit motormen for the electric railways was brought to my attention from without. The accidents which occurred through the fault, or at least not without the fault, of the motormen in street railway transportation have always aroused disquietude and even indignation in the public, and the street railway companies suffered much from the many payments of indemnity imposed by the court as they amounted to thirteen per cent of the gross earnings of some companies. Last winter the American Association for Labor Legislation called a meeting of vocational specialists to discuss the problem of these accidents under various aspects. The street railways of various cities were represented, and economic, physiological, and psychological specialists took part in the general discussion. Much attention was given, of course, to the questions of fatigue and to the statistical results as to the number of accidents and their relation to the various hours of the day and to the time of labor. But there was a strong tendency

to recognize, as still more important than the mere fatigue, the whole mental constitution of the motormen. The ability to keep attention constant, to resist distraction by chance happenings on the street, and especially the always needed ability to foresee the possible movements of the pedestrians and vehicles were acknowledged as extremely different from man to man. The companies claimed that there are motormen who practically never have an accident, because they feel beforehand even what the confused pedestrian and the unskilled chauffeur will do, while others relatively often experience accidents of all kinds because they do not foresee how matters will develop. They can hardly be blamed, as they were not careless, and yet the accidents did result from their personal qualities; they simply lacked the gift of instinctive foresight. All this turned the attention more and more to the possibilities of psychological analysis, and the Association suggested that I undertake an inquiry into this interesting problem with the means of the psychological laboratory. I felt the practical importance of the problem, considering that there are electric railway companies in this country which have up to fifty thousand accident indemnity cases a year. It therefore seemed to me decidedly worth while to undertake a laboratory investigation.

It would have been quite possible to treat the functions of the motormen according to the method which resolves the complex achievement into its various elements and tests every function independently. For instance, the stopping of the car as soon as the danger of an accident threatens is evidently effective only if the movement controlling the lever is carried out with sufficient rapidity. We should accordingly be justified in examining the quickness with which the individual reacts on optical stimuli. If a playing child suddenly runs across the track of the electric railway, a difference of a tenth of a second in the reaction-time may decide his fate. But I may say at once that I did not find characteristic differences in the rapidity of reaction of those motormen whom the company had found reliable and those who have frequent accidents. It seems that the slow individuals do not remain in the service at all. As a matter of course certain other indispensable single functions, like sharpness of vision, are examined before the entrance into the service and so they cannot stand as characteristic conditions of good or bad service among the actual employees.

For this reason, in the case of the motormen I abstracted from the study of single elementary functions and turned my attention to that mental process which after some careful observations

seemed to me the really central one for the problem of accidents. I found this to be a particular complicated act of attention by which the manifoldness of objects, the pedestrians, the carriages, and the automobiles, are continuously observed with reference to their rapidity and direction in the quickly changing panorama of the street. Moving figures come from the right and from the left toward and across the track, and are embedded in a stream of men and vehicles which moves parallel to the track. In the face of such manifoldness there are men whose impulses are almost inhibited and who instinctively desire to wait for the movement of the nearest objects; they would evidently be unfit for the service, as they would drive the electric car far too slowly. There are others who, even with the car at high speed, can adjust themselves for a time to the complex moving situation, but whose attention soon lapses, and while they are fixating a rather distant carriage, may overlook a pedestrian who carelessly crosses the track immediately in front of their car. In short, we have a great variety of mental types of this characteristic unified activity, which may be understood as a particular combination of attention and imagination.

My effort was to transplant this activity of the motormen into laboratory processes. And here

I may include a remark on the methodology of psychological industrial experiments. One might naturally think that the experience of a special industrial undertaking would be best reproduced for the experiment by repeating the external conditions in a kind of miniature form. That would mean that we ought to test the motormen of the electric railway by experiments with small toy models of electric cars placed on the laboratory table. But this would be decidedly inappropriate. A reduced copy of an external apparatus may arouse ideas, feelings, and volitions which have little in common with the processes of actual life. The presupposition would be that the man to be tested for any industrial achievement would have to think himself into the miniature situation, and especially uneducated persons are often very unsuccessful in such efforts. This can be clearly seen from the experiences before naval courts, where it is usual to demonstrate collisions of ships by small ship models on the table in the courtroom. Experience has frequently shown that helmsmen, who have found their course a life long among real vessels in the harbor and on the sea, become entirely confused when they are to demonstrate by the models the relative positions of the ships. Even in the naval war schools where the officers play at war with small model ships,

a certain inner readjustment is always necessary for them to bring the miniature ships on the large table into the tactical game. On the water, for instance, the naval officer sees the far-distant ships very much smaller than those near by, while on the naval game table all the ships look equally large. On the whole, I feel inclined to say from my experience so far that experiments with small models of the actual industrial mechanism are hardly appropriate for investigations in the field of economic psychology. The essential point for the psychological experiment is not the external similarity of the apparatus, but exclusively the inner similarity of the mental attitude. The more the external mechanism with which or on which the action is carried out becomes schematized, the more the action itself will appear in its true character.

In the method of my experiments with the motormen, accordingly, I had to satisfy only two demands. The method of examination promised to be valuable if, first, it showed good results with reliable motormen and bad results with unreliable ones; and, secondly, if it vividly aroused in all the motormen the feeling that the mental function which they were going through during the experiment had the greatest possible similarity with their experience on the front platform of the elec-

tric car. These are the true tests of a desirable experimental method, while it is not necessary that the apparatus be similar to the electric car or that the external activities in the experiment be identical with their performance in the service. After several unsatisfactory efforts, in which I worked with too complicated instruments, I finally settled on the following arrangement of the experiment which seems to me to satisfy those two demands.

The street is represented by a card 9 half-inches broad and 26 half-inches long. Two heavy lines half an inch apart go lengthwise through the centre of the card, and accordingly a space of 4 half-inches remains on either side. The whole card is divided into small half-inch squares which we consider as the unit. Thus there is in any cross-section 1 unit between the two central lines and 4 units on either side. Lengthwise there are 26 units. The 26 squares which lie between the two heavy central lines are marked with the printed letters of the alphabet from A to Z. These two heavy central lines are to represent an electric railway track on a street. On either side the 4 rows of squares are filled in an irregular way with black and red figures of the three first digits. The digit 1 always represents a pedestrian who moves just one step, and that

means from one unit into the next; the digit 2 a horse, which moves twice as fast, that is, which moves 2 units; and the digit 3 an automobile which moves three times as fast, that is, 3 units. Moreover, the black digits stand for men, horses, and automobiles which move parallel to the track and cannot cross the track, and are therefore to be disregarded in looking out for dangers. The red digits, on the other hand, are the dangerous ones. They move from either side toward the track. The idea is that the man to be experimented on is to find as quickly as possible those points on the track which are threatened by the red figures, that is, those letters in the 26 track units at which the red figures would land, if they make the steps which their number indicates. A red digit 3 which is 4 steps from the track is to be disregarded, because it would not reach the track. A red digit 3 which is only 1 or 2 steps from the track is also to be disregarded, because it would cross beyond the track, if it took 3 steps. But a red 3 which is 3 units from the track, a red 2 which is 2 units from the track, and a red 1 which is 1 unit from the track would land on the track itself; and the aim is quickly to find these points. The task is difficult, as the many black figures divert the attention, and as the red figures too near or too far are easily confused

with those which are just at the dangerous distance.

As soon as this principle for the experiment was recognized as satisfactory, it was necessary to find a technical device by which a movement over this artificial track could be produced in such a way that the rapidity could be controlled by the subject of the experiment and at the same time measured. Again we had to try various forms of apparatus. Finally we found the following form most satisfactory. Twelve such cards, each provided with a handle, lie one above another under a glass plate through which the upper card can be seen. If this highest card is withdrawn, the second is exposed, and from below springs press the remaining cards against the glass plate. The glass plate with the 12 cards below lies in a black wooden box and is completely covered by a belt 8 inches broad made of heavy black velvet. This velvet belt moves over two cylinders at the front and the rear ends of the apparatus. In the centre of the belt is a window 4½ inches wide and 2½ inches high. If the front cylinder is turned by a metal crank, the velvet belt passes over the glass plate and the little window opening moves over the card with its track and figures. The whole breadth of the card, with its central track and its 4 units on either side, is visible through it over

an area of 5 units in the length direction. If the man to be experimented on turns the crank with his right hand, the window slips over the whole length of the card, one part of the card after another becomes visible, and then he simply has to call the letters of those units in the track at which the red figures on either side would land, if they took the number of steps indicated by the digit. At the moment the window has reached Z on the card, the experimenter withdraws that card and the next becomes visible, as a second window in the belt appears at the lower end when the first disappears at the upper end. In this way the subject can turn his crank uninterruptedly until he has gone through the 12 cards. The experimenter notes down the numbers of the cards and the letters which the subject calls. Besides this, the number of seconds required for the whole experiment, from the beginning of the first card to the end of the twelfth, is measured with a stopwatch. This time is, of course, dependent upon the rapidity with which the crank is turned. The result of the experiment is accordingly expressed by three figures, the number of seconds, the number of omissions, that is, of places at which red figures would land on the track which were not noticed by the subject; and, thirdly, the number of incorrect places where letters were called in spite

of the fact that no danger existed. In using the results, we may disregard this third figure and give our attention to the speed and the number of omissions.

The necessary condition for carrying out the experiments with this apparatus is a careful, quiet, practical explanation of the device. The experiment must not under any circumstances be started until the subject completely understands what he has to do and for what he has to look out. For this purpose I at first always show the man one card outside of the apparatus and explain to him the differences between the black and the red figures, and the counting of the steps, and show to him in a number of cases how some red figures do not reach the track, how others go beyond the track, and how some just land in danger on the track. As soon as he has completely understood the principle; we turn to the apparatus and he moves the window slowly over a test card and tries to find the dangerous spots, and I turn his attention to every case in which he has omitted one or has given an incorrect letter. We repeat this slowly until he completely masters the rules of the game. Only then is he allowed to start the experiment. I have never found a man with whom this preparation takes more than a few minutes.

After developing this method in the psychological laboratory, I turned to the study of men actually in the service of a great electric railway company which supported my endeavors in the most cordial spirit. In accordance with my request, the company furnished me with a number of the best motormen in its service, men who for twenty years and more had performed their duties practically without accidents, and, on the other hand, with a large number of motormen who had only just escaped dismissal and whose record was characterized by many more or less important collisions or other accidents. Finally, we had men whose activity as motormen was neither especially good nor especially bad.

The test of the method lies first in the fact that the tried motormen agreed that they really pass through the experiment with the feeling which they have on their car. The necessity of looking out in both directions, right and left, for possible obstacles, of distinguishing those which move toward the track from the many which move along the track, the quick discrimination among the various rates of rapidity, the steady forward movement of the observation point, the constant temptation to give attention to those which are still too far away or to those which are so near that they will cross the track before the approach

of the car, in short, the whole complex situation with its demands on attention, imagination, and quick adjustment, soon brings them into an attitude which they themselves feel as identical with that in practical life. On the other hand, the results show a far-reaching correspondence between efficiency in the experiment and efficiency in the actual service. With a relatively small number of experiments this correspondence cannot be expected to be complete, the more as a large number of secondary features must enter which interfere with an exact correlation between experiment and standing in the railway company. We must consider, for instance, that those men whom the company naturally selects as models are men who have had twenty to thirty years of service without accidents, but consequently they are rather old men, who no longer have the elasticity of youth and are naturally less able to think themselves into an artificial situation like that of such an experiment, and who have been for a long time removed from contact with book work. It is therefore not surprising, but only to be expected, that such older, model men, while doing fair work in the test, are yet not seldom far surpassed by bright, quick, young motormen who are twenty years younger, even though they are not yet ideal motormen. Moreover, the standing in the

company often depends upon features which have nothing to do with the mental make-up of the man, while the experiment has to be confined to those mental conditions which favor accidents. It is quite possible that a man may happen to experience a slight collision, even though no conditions for the accident were lying in his mental make-up. But we may go still further. The experiment refers to those sides of his mind which make him able to foresee the danger points, and that is decidedly the most essential factor and the one from which most can be hoped for the safety of the public. But this does not exclude the possibility that some other mental traits may become causes of accidents. The man may be too daring and may like to run risks, or he may still need discipline, or he may not be sufficiently acquainted with the local conditions. Any such secondary factors may cause some slight accidents with the man who shows rather fair results in the experimental test of his foresight. Finally, we must not forget that some men enter into such tests under a certain nervous tension and therefore may not show so well at the very first test as their mental equipment should allow. Hence it is decidedly desirable not to rely on the first test, but to repeat it. If those various interferences are taken into account, the correspondence

between efficiency and the results of the tests is fairly satisfactory. It justified me in proposing that the experiments be continued and in regarding it as quite possible that later tests on the basis of this principle may be introduced at the employment of motormen.

A difficulty is presented by the valuation of the numerical results. The mere number of omissions alone cannot be decisive, as it is clear that no intelligent man would make any omissions if he should give an unlimited amount of time to it; for instance, if he were to spend fifteen minutes on those 12 cards. But this is the same thing as to say that a motorman would not run over any one if he were to drive his car one mile in an hour. The practical problem is to combine the greatest possible speed with the smallest number of oversights, and both factors must therefore be considered. The subject who makes relatively many mistakes but uses a very short time must be acknowledged to be as good as the man who makes fewer mistakes but takes a longer time. In the results which I have gathered in experiments with motormen, no one has gone through those 12 cards in a shorter time than 140 seconds, while the longest time was 427 seconds. On the other hand, no one of the motormen made less than 4 omissions, while the worst ones made 28

omissions. I abstract from one extreme case with 36 omissions. On the whole, we may say that the time fluctuates between 180 and 420, the mistakes between 4 and 28. The aim is to find a formula which gives full value to both factors and makes the material directly comparable in the form of one numerical value instead of the two. If we were simply to add the number of seconds and the number of omissions, the omissions would count far too little, inasmuch as 10 additional omissions would then mean no more than 10 additional seconds. On the other hand, if we were to multiply the two figures the omissions would mean by far too much, as the transition from 4 mistakes to 8 mistakes would then be as great a change as the transition from 200 to 400 seconds, that is, from the one extreme of time to the other. Evidently we balance both factors if we multiply the number of omissions by 10 and add them to the number of seconds. The variations between 4 and 28 omissions are 24 steps, which multiplied by 10 correspond to the 240 steps which lie between 180 and 420 seconds. On that basis any additional 50 seconds would be equal to 5 additional omissions. If of two men one takes 100 seconds less than his neighbor, he is equal to him in his ability to satisfy the demands of the service, if he makes 10 mistakes more.

On the basis of this calculation I find that the old, well-trained motormen come to a result of about 450, and I should consider that an average standard. This would mean that a man who uses 400 seconds would not be allowed to make more than 5 omissions, in 350 seconds not more than 10, in 300 not more than 15, in 250 not more than 20, under the condition that these are the results of the first set of experiments. Where there are more than 20 omissions made, mere quickness ought not to be allowed as a substitute. The man who takes 150 seconds and makes 30 mistakes would come up to the same standard level of 450. Yet his characteristics would probably not serve the interests of the service. He would speed up his car and would make better time than any one else, but would be liable to accidents. I should consider 20 mistakes with a time not longer than 250 as the permissible maximum. Among the younger motormen whom I examined, the best result was 290, in which 270 seconds were used and only 2 omissions made. Results below 350 may be considered as very good. One man, for instance, carried out the experiment in 237 seconds with 11 mistakes, which gives the result 347. From 350 to 450 may be counted as fair, 450 to 550 as mediocre, and over 550 as very poor. In the case of old men, who may

be expected to adjust themselves less easily to artificial experiments, the limits may be shifted. If the experiments are made repeatedly, the valuation of the results must be changed accordingly. The training of the men in literary and mathematical work or in experimentation may be considered, as our experiments have shown that highly educated young people with long training in experimental observations can pass through the test much more quickly than any one of the motormen could. Among the most advanced graduate students who do research work in my Harvard laboratory there was no one whose result was more than 275, while, as I said, among all the motormen there was no one whose result was less than 290. The best result reached was by a student who passed through the test in 223 seconds with only 1 mistake, the total therefore being 233. Next came a student who did it in 215 seconds with 3 mistakes, total, 245; then in 228 seconds with 2 omissions, total, 248, and so on.

I recapitulate: With men on the educational level and at the age that comes in question for their first appointment in the service of an electric railway company, the test proposed ought to be applied according to this scheme. If they make more than 20 mistakes, they ought to be excluded; if they make less then 20 mistakes, the number of

omissions is to be multiplied by 10 and added to the number of seconds. If the sum is less than 350, their mental fitness for the avoidance of accidents is very high, between 350 and 450 fair, and more than 550 not acceptable under any conditions. I submit this, however, with the emphasis on my previous statement that the investigation is still in its first stage, and that it will need a long coöperation between science and industry in order to determine the desirable modifications and special conditions which may become necessary in making the employment of men partly dependent upon such psychological tests. There can be no doubt that the experiments could be improved in many directions. But even in this first, not adequately tested, form, an experimental investigation of this kind which demands from each individual hardly 10 minutes would be sufficient to exclude perhaps one fourth of those who are nowadays accepted into the service as motormen. This 25 per cent of the applicants do not deserve any blame. In many other occupations they might render excellent service; they are neither careless nor reckless, and they do not act against instructions, but their psychical mechanism makes them unfit for that particular combination of attention and imagination which ought to be demanded for the special task of the motor-

man. If the many thousands of injury and the many hundreds of death cases could be reduced by such a test at least to a half, then the conditions of transportation would have been improved more than by any alterations in the technical apparatus, which usually are the only objects of interest in the discussion of specialists. The whole world of industry will have to learn the great lesson, that of the three great factors, material, machine, and man, the man is not the least, but the most important.

IX

EXPERIMENTS IN THE INTEREST OF SHIP SERVICE

WHERE the avoidance of accidents is in question, the test of a special experimental method can seldom be made dependent upon a comparison with practical results, as we do not want to wait until the candidate has brought human life into danger. The ordinary way of reaching the goal must therefore be an indirect one in such cases. For the study of motormen the conditions are exceptionally favorable, as hundreds of thousands of accidents occur every year, but another practical example may be chosen from a field where it is, indeed, impossible to correlate the results with actual misfortunes, because the dangerous situations occur seldom; and nevertheless on account of their importance they demand most serious study. I refer to the ship service, where the officer on the bridge may bring thousands into danger by one single slip of his mind. I turn to this as a further concrete illustration in order to characterize at once the lengths to which such vocational studies may advance.

One of the largest ship companies had ap-

proached me — long before the disaster of the Titanic occurred — with the question whether it would not be possible to find psychological methods for the elimination of such ship officers as would not be able to face an unexpected suddenly occurring complication. The director of the company wrote to me that in his experience the real danger for the great ships lies in the mental dispositions of the officers. They all know exactly what is to be done in every situation, but there are too many who do not react in the appropriate way when an unexpected combination of factors suddenly confronts them, such as the quick approach of a ship in the fog. He claimed that two different types ought to be excluded. There are ship officers who know the requirements excellently, but who are almost paralyzed when the dangerous conditions suddenly threaten. Their ability for action is inhibited. In one moment they want to act under the stimulus of one impression, but before the impulse is realized, some other perhaps rather indifferent impression forces itself on their minds and suggests the counteraction, and in this way they vacillate and remain inactive until it is too late to give the right order or to press the right button. The other type feels only the necessity for rapid action, and under the pressure of greatest haste, without clear thought, they

jump to the first decision which rushes to their minds. Without carefully considering the conditions really given, they explode in an action which they would never have chosen in a state of quiet deliberation. They react on any accidental circumstance, just as at a fire men sometimes carry out and save the most useless parts of their belongings. Of course, beside these two types, there is the third type, the desirable one, the men who in the unexpected situation quickly review the totality of the factors in their relative importance and with almost instinctive certainty immediately come to the same decision to which they would have arrived after quiet thought. The director of the company insisted that it would be of highest importance for the ship service to discriminate these three types of human beings, and to make sure that there stand on the bridge of the ship only men who do not belong to those two dangerous classes. He turned to me with this request, as he had heard of the work toward economic psychology in the Harvard laboratory.

As the problem interested me, I carried on a long series of experiments in order to construct artificial conditions under which the mental process of decision in a complicated situation, especially the rapidity, correctness, and constancy of the decision, could be made measurable. I started

from the conviction that this complex act of decision must stand in definite relation to a number of simpler mental functions. If, for instance, it stood in a clear definite relation to the process of association, or discrimination, or suggestibility, or perception, or memory, and so on, it would be rather easy to foresee the behavior of the individual in the act of decision, as every one of those other simple mental functions could be tested by routine methods of the psychological laboratory. This consideration led me to propose ramified investigations concerning the psychology of decision in its relation to the elementary mental processes. These studies by students of the laboratory are not yet completed. But I soon saw that they would be unfit for the solution of my practical problem, as we recognized that these relations between the complex act of decision and the elementary functions of the individual seem to have different form with different types of men.[12] If I was to approach the solution of the practical problem, accordingly, I had to reproduce in an experimental form the act of decision under complex conditions.

It seemed necessary to create a situation in which a number of quantitatively measurable factors were combined without any one of them forcing itself to consciousness as the most important.

The subject to be experimented on then has to decide as quickly as possible which of the factors is the relatively strongest one. As usual, here, too, I began with rather complicated material and only slowly did I simplify the apparatus until it finally took an entirely inconspicuous form. But this is surely the most desirable outcome for testing methods which are to be applied to large numbers of persons. Complicated instruments, for the handling of which special training is needed, are never so useful for practical purposes as the simple schemes which can be easily applied. The form of which I finally made use is the following. I work with 24 cards of the size of playing-cards. On the upper half of every one of these cards we have 4 rows of 12 capital letters, namely, A, E, O, and U in irregular repetition. On 4 cards, one of these vowels appears 21 times and each of the three others 9 times; on 8 cards, one appears 18 times and every one of the three others 10 times; on 8 cards, one appears 15 times and each of the others 11 times; and finally, on 4 cards one vowel appears 16 times, each of the three others 8 times, and besides them 8 different consonants are mixed in. The person to be tested has to distribute these 24 cards as quickly as possible in 4 piles, in such a way that in the first pile are placed all cards in which the letter A is most frequent, in the second

those in which the letter E predominates, and so on. As a matter of course the result must never be secured by counting the letters. Any attempt to act against this prescription and secretly to begin counting would moreover delay the decision so long that the final result would be an unsatisfactory achievement anyhow. It would accordingly bring no advantage to the candidate.

We measure with a stopwatch in fifths of a second the time for the whole process from the subject's looking at the first card to his laying down of the last card, and, secondly, we record the number and the character of his mistakes, if cards are put into wrong piles. I have made the experiment with very many persons, and results show that those various mental traits which have been observed in the practical ship service come clearly to light under the conditions of this experiment. Some of the persons lose their heads entirely, and for many of them it is a painful activity for which they require a long time. Even if the number of mistakes is not considerable, they themselves have the feeling that they are not coming to a satisfactory decision, because their attention is pulled hither and thither so that they feel an inner mental paralysis. Some chance letters stand out and appear to them to be predominant, but in the next moment the attention is

captured by some other letters which bring the suggestion that they are in the majority and that they present the most important factor. The outcome is that inner state of indecision which can become so fatal in practical life. Other subjects distribute the cards in piles at a relatively high speed, and they do it with the subjective feeling that they have indeed recognized at the first glance the predominant group of letters. The exact measurement of the results, however, shows that they commit many errors which would have been improbable after quiet consideration. Any small group of letters which catches their eye makes on them, under the pressure of their haste, such a strong impression that all the other letters are inhibited for the moment and the wrong decision is quickly made. Finally, we find a group of persons who carry out the experiment rather quickly and at the same time with few mistakes. It is characteristic of them to pass through it with the feeling that it is an agreeable and stimulating mental activity. In all cases the subjects feel themselves under the unified impression which results from all those 48 letters of the card together; and this is the reason why the qualitative manifoldness of a practical life situation can be compared with these intermingled, quantitatively determined groups of letters.

If I consider the general results of these experiments only with reference to the time-measurement, I should say that a person who completes the distribution of the cards in less than 80 seconds is quick in his decisions; from 80 to 150, moderately quick; from 150 to 250, slow and deliberate and rather too deliberate for situations which demand quick action; over 250 seconds, he would belong among those wavering persons who hesitate too long in a life situation which demands decision. The time which is needed for the mere distribution of the cards themselves plays a very small rôle compared with the time of the whole process, and can be neglected. In order to determine this, I asked all the subjects before they made the real experiment to distribute 24 other cards in 4 piles, on each of which one of the four letters, A, E, O, and U was printed only once. Hence no comparison of various factors was involved in this form of distribution. The average time for this ordinary sorting was about 20 seconds. Only rather quick individuals carried it out in less than 18 and only very slow ones needed more than 25 seconds. This maximum variation of 10 seconds is evidently insignificant, as the variations in the experiment amount to more than 200 seconds. But it is very characteristic that the results of the two experiments do not move parallel. Some

persons, who are able to sort the cards on which only one of the 4 letters is printed very quickly, are rather slow when they sort the cards with the 48 letters for which the essential factor is the act of comparison. In the first case the training in card-playing also seems to have a certain influence, but in the second case, our real experiment on decision, this influence does not seem to exist.

We have emphasized from the start that it is no less important to give consideration to the number of mistakes. A mere rapidity of distribution with many mistakes characterizes, as we saw, a mental system which is just as unfit for practical purposes as one which acts with too great slowness. But it would not have been sufficient simply to ask how many cards were put into wrong piles. The special arrangement of the cards with four different types of combinations was introduced for the purpose of discriminating among mistakes of unequal seriousness. When one letter appeared 21 times and the three others only 9 times, it was surely much easier to make the decision than when the predominant letter appeared only 15 times and the other three each 11 times. The easier the right decision, the graver the mistake. Of course the valuation of these mistakes must be rather arbitrary. We decided to value as 4 every mistake in these cards on which the predominant

letter appears 21 times; as 3, a mistake in the 18 letter cards; as 2, a mistake in the 16 letter cards; and as 1, a mistake in the most difficult ones, the 15 letter cards. If the mistakes are calculated on this basis and are added together, a sum below 5 may indicate a very safe and perfectly reliable ability for decision; 5 to 12, satisfactory; 12 to 20, uncertain; and over 20, very poor. In order to take account of both factors, time and mistakes, we multiply the sum of the calculated mistakes by the number of seconds. If the product of these two figures is less than 400, it may be taken as a sign of perfect reliability in making very quick, correct decisions, in complex life situations; 400 to 1000 indicates the limits between which the ability for such decisions may be considered as normal and very satisfactory; 1000 to 2000, not good but still adequate; 2000 to 3000, unreliable, and over 3000, practically absent. It is clear that the real proof of the value of this method cannot be offered. This is just the reason why we selected this illustration as an example of the particular difficulty. Wrong decisions, that is, cases in which the man on the bridge waits too long before he makes his decision and thus causes a collision of ships by his delay, or in which he rushes blindly to a decision which he himself would have condemned after quiet deliberation, are

rare. It would be impossible to group such men together for the purpose of the experiment and to compare their results with those of model captains, the more as experience has shown that an officer may have a stainless record for many years and yet may finally make a wrong decision which shows his faulty disposition. The test of the method must therefore be a somewhat indirect one. My aim was to compare the results of the experiments with the experiences of the various individuals which they themselves reported concerning their decisions in unexpected complicated situations, and moreover with the judgments of their friends whom I asked to describe what they would expect from the subjects under such conditions. The personal differences in these respects are extremely great, and are also evident in the midst of small groups of persons who may have great similarity in their education and training and in many other aspects of their lives.

Among the most advanced students of my research laboratory, for instance, all of whom have rather similar schooling and practically the same training in experimental work, the product of mistakes and seconds varied between 348 and 13,335. That smallest value occurred in a case in which the time was 116 seconds and the sum of the mistakes only 3, inasmuch as 3 cards of the most dif-

ficult group where the predominating letter occurred only 15 times were put in the wrong piles. The shortest time among my laboratory students was 58 seconds, but with this individual the sum of the mistakes, calculated on the basis of the valuation agreed upon, was 13. The largest figure mentioned resulted in a case in which the student needed 381 seconds and yet made mistakes the sum of which amounted to 35. It is characteristic that the person with the smallest product felt a distinct joy in the experiment, while the one with the largest passed through painful minutes which put him to real organic discomfort. If we arrange the men simply in the order of these products, of course we cannot recognize the various groups, as those who are quick but make mistakes and those who make few mistakes but act slowly may be represented by the same products. The coincidence of the results with the self-characterization is frequently quite surprising. Every one has at some time come into unexpected, suddenly arising situations and many have received in such moments a very vivid impression of their own mental reaction. They know quite well that they could not come to a decision quickly enough, or that they rushed hastily to a wrong decision, or that in just such instants a feeling of repose and security came over them and that with sure instinct they

turned in the direction which they would have chosen after mature thought. The results of the experiments in sorting the cards confirmed this self-observation in such frequent cases that it may indeed be hoped that a more extended test of this method will prove its practical usefulness. It is clear that the field is a wide one, as these different types of mental dispositions must be of consequence not only in the ship service, but also to a certain degree in the railroad service and in many other industrial tasks.

We have emphasized from the start that as a matter of course such a tested function, while it is taken in its complex unity, is nevertheless not the only psychophysical disposition of significance. This is as true for the ship officer as it was for the motorman of the electric car. If we were to study all the mental dispositions necessary or desirable for the ship officer, we should find many other qualities which are accessible to the psychological investigation. The captain of the ship, for instance, is expected to recognize the direction of a vessel passing in the fog by the signals of the foghorn. But so far no one has given any attention to the psychological conditions of localization of sound, which were for a long while a much-studied problem of our psychological laboratories. We know how this localization is dependent upon

the comparison of the two ears and what particular mistakes occur from the different sensibility of the two ears. Yet there are to-day men on the bridges of the ships who hear much better with one ear than with the other, but who still naïvely believe that, as they hear everything very distinctly with one ear, this normal ear is also sufficient for recognizing the direction of the sound. It is the same mistake which we frequently see among laborers whose vision has become defective in one of their eyes, or one of whose eyes is temporarily bandaged. They are convinced that the one good eye is sufficient for their industrial task, because they are able to recognize everything clearly and distinctly. They do not know that both the eyes together are necessary in order to produce that psychological combination by which the visual impression is projected into the right distance, and that in the factory they are always in danger of underestimating the distance of a wheel or some other part of the machine and of letting the hand slip between the wheels or knives. The results of experimental psychology will have to be introduced systematically into the study of the fitness of the personality from the lowest to the highest technical activity and from the simplest sensory function to the most complex mental achievement.

X

EXPERIMENTS IN THE INTEREST OF TELEPHONE SERVICE

OUR plan was to illustrate the possibility of applying psychological experiments to the selection of fit applicants also in cases in which not one characteristic mental function stands out, but in which a large number of relatively independent mental activities are in play. I choose as an illustration of such cases the work of the employees at the telephone switchboard. A study of the psychological factors in this work is strongly suggested by the practical interests of the telephone companies, and may be looked on here exclusively from this point of view. The user of the telephone is little inclined to consider how many actions have to be carried out in the central office before the connection is made and finally broken again. From the moment when the speaker takes off the receiver to the cutting off of the connection, fourteen separate psychophysical processes are necessary in the typical case, and even then it is presupposed that the telephone girl understood the exchange and number correctly. It is a common experience of the companies that these

demands cannot be satisfactorily fulfilled when a telephone girl has to handle more than 225 calls in an hour. The official statistics show that this figure is exceeded in not infrequent cases,[13] in extreme cases the number may even rise beyond 300. Moreover, in short periods of reinforced demands it may happen that for a few minutes even the rapidity of 10 calls in a minute is reached. Normally the burden is divided among the employees in such a way that about 150 calls fall to each one in an hour, and that this figure is passed considerably only in one morning and one evening hour. A skillful distribution of pauses and ample arrangements for rest, usually together with very excellent hygienic conditions, make it possible for the fit persons to be able to carry on this work without over-fatigue from 8 to 9 hours a day. On the other hand, it is only natural that such rapid and yet subtle activity under such high tension, where especially the quick localization of the correct hole is a difficult and yet indispensable part, can be carried out only by a relatively small number of human nervous systems. The inability to keep attention at such a high point for a long while, or to perform such rapid movements, or to retain the numbers correctly, does not lead to fatal accidents like those in the case of the unfit motormen, but it does lead to fatigue and finally to

a nervous breakdown of the employees and to confusion in the service. The result is that the company is continually obliged to dismiss a considerable proportion of those who have entered the service and who have spent some months in going through the training school of the company. As one single company, the Bell Telephone Company, employs 16,000 operators, the problem is an expansive one, and it has bearing on the health of the employees as well as on the patience of the subscribers. But above all it refers to the economic interests of the company, inasmuch as every girl who satisfies the entrance conditions of hearing and sight, of school education and general personal appearance, receives some salary throughout the months of training in the telephone school. Since during the first half-year, in which the employee still works entirely under supervision, more than a third of those who had originally entered leave, partly on account of unfitness and inability, partly on account of over-fatigue or similar reasons, the economic disadvantage to the company is evidently a very great one. The candidates are paid for months of mere training, and they themselves waste their energy and time with practice in a kind of labor which cannot be serviceable to them in any other economic activity. Under these circumstances it is not sur-

prising that one city system approached me with the question whether it would not interest me from a scientific point of view to examine how far the mental fitness of the employees could be determined beforehand through experimental means.

After carefully observing the service in the central office for a while, I came to the conviction that it would not be appropriate here to reproduce the activity at the switchboard in the experiment, but that it would be more desirable to resolve that whole function into its elements and to undertake the experimental test of a whole series of elementary mental dispositions. Every one of these mental acts can then be examined according to well-known laboratory methods without giving to the experiments any direct relation to the characteristic telephone operation as such. I carried on the first series of experiments with about thirty young women who a short time before had entered into the telephone training school, where they are admitted only at the age between seventeen and twenty-three years. I examined them with reference to eight different psychophysical functions. In saying this, I abstract from all those measurements and tests which had somewhat anthropometric character, such as the measurement of the length of the fingers, the rapidity of breathing, the rapidity of pulse,

the acuity of vision and of hearing, the distinctness of the pronunciation, and so on. A part of the psychological tests were carried on in individual examinations, but the greater part with the whole class together.

These common tests referred to memory, attention, intelligence, exactitude, and rapidity. I may characterize the experiments in a few words. The memory examination consisted of reading to the whole class at first two numbers of 4 digits, then two of 5 digits, then two of 6 digits, and so on up to figures of 12 digits, and demanding that they be written down as soon as a signal was given. The experiments on attention, which in this case of the telephone operators seemed to me especially significant, made use of a method the principle of which has frequently been applied in the experimental psychology of individual differences and which I adjusted to our special needs. The requirement is to cross out a particular letter in a connected text. Every one of the thirty women in the classroom received the same first page of a newspaper of that morning. I emphasize that it was a new paper, as the newness of the content was to secure the desired distraction of the attention. As soon as the signal was given, each one of the girls had to cross out with a pencil every "a" in the text for six minutes. After a certain time, a

bell signal was given and each then had to begin a new column. In this way we could find out, first, how many letters were correctly crossed out in those six minutes, secondly, how many letters were overlooked, and, thirdly, how the recognition and the oversight were distributed in the various parts of the text. In every one of these three directions strong individual differences were indeed noticeable. Some persons crossed out many, but also overlooked many, others overlooked hardly any of the "a's," but proceeded very slowly so that the total number of the crossed-out letters was small. Moreover, it was found that some at first do poor work, but soon reach a point at which their attention remains on a high level; others begin with a relatively high achievement, but after a short time their attention flags, and the number of crossed-out letters becomes smaller or the number of unnoticed, overlooked letters increases. Fluctuations of attention, deficiencies, and strong points can be discovered in much detail.

The third test which was tried with the whole class referred to the intelligence of the individuals. Discussion of the question how to test intelligence in general would quickly lead us into as yet unsettled controversies. It is a chapter of the psychology of tests which, especially in the service of

pedagogy but to a certain degree also in the service of medicine, has been more carefully elaborated than any other. Often it has been contested whether we have any right to speak of one general central intelligence factor, and whether this apparently unified activity ought not to be resolved into a series of mere elementary processes. The newer pedagogical investigations, however, speak in favor of the view that besides all special processes, or rather, above all of them, an ability must be recognized which cannot be divided any further, and by which the individual adjusts his knowledge, his experiences, and his dispositions to the changing purposes of life. The grading of the pupils in a class usually expresses this differentiation of the intelligence; and while the differences of industry or of mere memory and similar secondary features may sometimes interfere, it remains after all not difficult for an observant teacher to grade the pupils of his class, whom he knows well, according to their general intelligence. The psychological experiments carried on in the schoolroom have demonstrated that this ability can be tested by the measurement of some very simple mental activities. The best method would be the one which would allow the experimenter, on the basis of a single experiment, to grade the individuals in the same order in which they

appear in the record of the teacher. Among the various proposed schemes for this purpose the figures suggest that the most reliable one is the following method, the results of which show the highest agreement between the rank order based on the experiments and the rank order of the teachers.[14] The experiment consists in reading to the pupils a long series of pairs of words of which the two members of the pair always logically belong together. Later, one word of each pair will be read to them and they have to write down the word which belonged with it in the pair. This is not a simple experiment on memory. The tests have shown that if instead of logically connected words simply disconnected chance words are offered and reproduced, no one can keep such a long series of pairs in mind, while with the words which have related meaning, the most intelligent pupils can master the whole series. The very favorable results which this method had yielded in the classroom made me decide to try it in this case too. I chose for an experiment 24 pairs of words from the sphere of experience of the girls to be tested. Two further class experiments belonged rather to the periphery of psychology. The exactitude of space-perception was measured by demanding that each divide first the long and then the short edge of a folio sheet into two equal

halves by a pencil mark. And finally, to measure the rapidity of movement, it was demanded that every one make with a pencil on the paper zigzag movements of a particular size during the ten seconds from one signal to another.

After these class experiments I turned to individual tests. First, every girl had to sort a pack of 48 cards into 4 piles as quickly as possible. The time was measured in fifths of a second. The following experiment which referred to the accuracy of movement impulses demanded that every one try to reach with the point of a pencil 3 different points on the table in the rhythm of metronome beats. On each of these three places a sheet of paper was fixed with a fine cross in the middle. The pencil should hit the crossing point, and the marks on the paper indicated how far the movement had fallen short of the goal. One of these movements demanded the full extension of the arm and the other two had to be made with half-bent arm. I introduced this last test because the hitting of the right holes in the switchboard of the telephone office is of great importance. The last individual experiment was an association test. I called six words like "book," "house," "rain," and had them speak the first word which came to their minds. The time was measured in fifths of a second only, as subtler experiments, for which

hundredths of a second would have to be considered, were not needed.

In studying the results so far as the memory experiments were concerned, we found that it would be useless to consider the figures with more than 10 digits. We took the results only of those with 8, 9, and 10 digits. There were 54 possibilities of mistakes. The smallest number of actual mistakes was 2, the largest, 29. In the experiment on attention made with the crossing-out of letters, we found that the smallest number of correctly marked letters was 107, the largest number in the six minutes, 272; the smallest number of overlooked letters was 2, the largest, 135; but this last case of abnormal carelessness stood quite isolated. On the whole, the number of overlooked letters fluctuated between 5 and 60. If both results, those of the crossed-out and those of the overlooked letters, are brought into relation, we find that the best results were a case of 236 letters marked, with only 2 overlooked, and one of 257 marked, with 4 overlooked. The very interesting details as to the various types of attention which we see in the distribution of mistakes over the six minutes were not taken into our final table. The word experiments by which we tested the intelligence showed that no one was able to reproduce more than 22 of the 24 words. The smallest num-

ber of words remembered was 7. The mistakes in the perception of distances fluctuated between 1 and 14 millimeters; the time for the sorting of the 48 cards, between 35 and 58 seconds; the association-time for the 6 associated words taken together was between 9 and 21 seconds. The pointing experiments could not be made use of in this first series, as it was found that quite a number of participants were unable to perform the act with the rapidity demanded.

Several ways were open to make mathematical use of these results. I preferred the simplest way. I calculated the grade of the girls for each of these achievements. The same candidate who stood in the 7th place in the memory experiment was in the 15th place with reference to the number of letters marked, in the 3rd place with reference to the letters overlooked, in the 21st place with reference to the number of word pairs which she had grasped, in the 11th place with reference to the exactitude of space-perception, in the 16th place with reference to the association-time, and in the 6th place with reference to the time of sorting. As soon as we had all these independent grades, we calculated the average and in this way ultimately gained a common order of grading. It is evident that this kind of calculation contains accidental factors, especially as a consequence of the fact that we

give equal value to every one of these results. It might be better, for instance, to attribute a higher value to the attention experiment or to the intelligence experiment. This could be done by multiplying the results of some of these grades by 2 or by 3, which would bring the high or low grade of a girl for a particular function to stronger influence in the final result. But in this first trial I contented myself with the simplest uniform scheme in order to exclude all arbitrariness, and therefore considered the mere average of all the grades as the expression of the experimental result.

With this average rank list, we compared the practical results of the telephone company after three months had passed. These three months had been sufficient to secure at least a certain discrimination between the best, the average, and the unfit. The result of this comparison was on the whole satisfactory. First, the skeptical telephone company had mixed with the class a number of women who had been in the service for a long while and had even been selected as teachers in the telephone school. I did not know, in figuring out the results, which of the participants in the experiments these particularly gifted outsiders were. If the psychological experiments had brought the result that these individuals who stood so high

in the estimation of the telephone company ranked low in the laboratory experiment, it would have reflected strongly on the reliability of the laboratory method. The results showed, on the contrary, that these women who had proved most able in practical service stood at the top of our list. Correspondingly, those who stood the lowest in our psychological rank list had in the mean time been found unfit in practical service and had either left the company of their own accord or else had been eliminated. The agreement, to be sure, was not a perfect one. One of the list of women stood rather low in the psychological list, while the office reported that so far she had done fair work in the service, and two others to whom the psychological laboratory gave a good testimonial were considered by the telephone office as only fair.

But it is evident that certain disagreements would have occurred even with a more ideal method, as on the one side no final achievement in practical service can be given after only three months, and because on the other side a large number of secondary factors may enter which entirely overshadow the mere question of psychophysical fitness. Poor health, for instance, may hinder even the most fit individual from doing satisfactory work, and extreme industry and ener-

getic will may for a while lead even the unfit to fair achievement, which, to be sure, is likely to be coupled with a dangerous exhaustion. The slight disagreements between the psychological results and the practical valuation, therefore, do not in the least speak against the significance of such a method. On the other hand, I emphasize that this first series meant only the beginning of the investigation, and it can hardly be expected that at such a first approach the best and most suitable methods would at once be hit upon. A continuation of the work will surely lead to much better combinations of test experiments and to better adjusted schemes. But it would be most desirable that such studies be undertaken at various places according to various schemes in order to come nearer to the solution of a problem which is economically important to the whole public and to many thousands of employees. As soon as methods are really perfected it would seem not at all impossible that by a short experiment of a few minutes thousands of applicants might be saved long months of study and training which are completely wasted. For us here the detailed analysis of this particular case did not mean a suggestion to use to-day in the telephone offices of the country the special scheme which we applied, but it stood only as a clear,

simple illustration of a method by which not the specific work itself is tested, but by which the industrial work of the individual is resolved into a long series of parallel functions each one of which is tested independently. The experimental aid which the laboratory has to supply in such cases is not a newly invented device, such as we needed in the case of the motormen, but simply the methods well known as so-called mental tests.

The experiments with such tests by which single mental functions are measured approximately in short quick examinations, has been much discussed in psychological circles. For a long while the thorough scholars remained very reluctant to accept such an apparently superficial scheme, when these tests were proposed especially for the pedagogical interests of the schoolroom. It was a time in which the scientific efforts were completely devoted to the general problems of the human mind and in which individual differences were very little considered. Moreover, the questions of applied psychology still seemed so far distant that the true scholar instinctively took his standards from the methods of purely theoretical research. Seen from such a point of view, it could not be denied that the tests were not sufficient to give us a complete scientific analysis of

the personality in its subtler structure. The theorists knew too well that if the reactions, or associations, or memories, or tendencies of attention, or emotions of a subject were measured really with that scientific thoroughness which is the ideal of research, long months of experiments would be needed, and little could be hoped for from tests to be performed in half an hour. But this somewhat haughty reserve which was quite justified twenty years ago has become obsolete and would be meaningless to-day. On the one side the methods themselves have been multiplied; for each mental act like memory, attention, and so on, dozens of well-studied tests are at our disposal, which are adjusted to the finest ramifications of the functions.[15] On the other side the interest in individual differences and in applied psychology has steadily grown, and through it an understanding for the real meaning of the tests has been gained. Their value, indeed, lies exclusively in their relation to the practical problems. Where theoretical questions are to be answered and scientific studies concerning the laws and variations of the mind are to be undertaken, the long series of laboratory experiments carried on with patience and devotion are indispensable and can never be replaced by the short-cut methods of the tests. But where practical tasks of

pedagogy or jurisprudence or medicine, or especially of commerce and industry, are before us, the method of tests ought to be sovereign. It can be adapted to the special situations and can succeed perfectly, if the task is to discover the outlines of the mental individuality for particular practical work.

The only real difficulty of the method lies in the ease with which it can be used. A device which presupposes complicated instruments deters the layman and will be used only by those who are well trained. Moreover, the amateur would not think of constructing and adapting such apparatus himself. But when nothing is necessary but to use words or numbers or syllables or pictures, or, as in those experiments which we just described, newspapers and so on, any one feels justified in applying the scheme or in replacing it by a new apparently better one according to his caprice. The manifoldness of the proposed tests for special functions is therefore enormous to-day. What is needed now is surely much more that order be brought into this chaos of propositions, and that definite norms and standards be secured for certain chief examinations, than that the number of variations simply be increased.

The chief danger, moreover, lies in the fact that those who are not accustomed to psychological

laboratory research are easily misled. They fancy that such an experiment can be carried out in a mere mechanical way without careful study of all the conditions and accompanying circumstances. Thereby a certain crudeness of procedure may enter which is not at all suggested by the test method itself. The psychological layman too seldom recognizes how many other psychical functions may play a rôle in the result of the experiment beside the one which is interesting him at that moment. The well-schooled laboratory worker almost automatically gives consideration to all such secondary circumstances. While his experiments may refer to the process of memory, he will yet at the same time carefully consider the particular situation as to the emotional setting of the subject, as to his attention, as to his preceding experience, as to his intelligence, as to his physiological condition, and many other factors which may have indirect influence even on the simplest memory test. Hence the real performance of the experiments ought to be undertaken only by those who are thoroughly familiar and well trained in psychological research. And they alone, moreover, can decide what particular form such an experiment ought to take in a given practical situation. It must be left to them, for instance, to judge in which cases the mental func-

tion of economic importance ought to be tested after being resolved into its components and in which it ought to be examined in its characteristic unity.

XI

CONTRIBUTIONS FROM MEN OF AFFAIRS

WHILE the psychologists have to perform the actual labor, the representatives of practical life are much better able to indicate the points at which the psychological levers ought to be applied. In the past year I have sought contact with several hundred large concerns in America which belong to many different industrial realms. My time did not allow me personal observation in so many cases, but everywhere I begged for information from the leading men. I asked in individual letters for the particular psychological qualities which from the standpoint of the management seemed essential for the various kinds of labor in their establishments. I always inquired to what extent consideration was given to such psychological points of view at the appointment of applicants, and asked for material concerning the question how far individuals who proved to be unfit for one kind of labor showed fitness at other kinds of work. The replies which I received from all sides varied from a few meaningless lines to long documents, which in some cases were composed of detailed reports from all the department chiefs of a particular concern.

The common fundamental turn was decidedly a feeling of strong interest in the formulation of the question, which was practically new to all of them. Whether the answer came from paper mills or machine shops, from meat-packing houses or from breweries, from electrical or chemical mills, from railroad or mining companies, from department stores or from publishing houses, everywhere it was acknowledged that they had given hardly any conscious attention to the real psychological dispositions of their employees. They had of course noticed whether their men were industrious or lazy, honest or dishonest, skillful or clumsy, peaceful or quarrelsome people, but I had emphasized from the start in all my letters that such points of view were not before my mind. The mental qualities for which I asked were the psychological functions of attention, memory, ideas, imagination, feeling, volition, suggestibility, ability to learn, ability to discriminate, judgment, space-sense, time-sense, and so on. It would lead too far here to discuss why these two groups of characteristics indeed belong to two different aspects of mental life, and why only the latter is strictly psychological. The way in which the management is accustomed to look on their men is the practical way of ordinary life, in which we try to understand our neighbor by entering

into the meaning of his mental functions and by seeking to grasp what his aim is. But such an interpretation of the other man's mind is not a psychological analysis. It gives us the purposes of his inner life, but does not show us its structure and its component parts. We can abstract from interpreting and appreciating in order to describe the elements of the mind which in themselves have no meaning and no value, but which are the only important factors, if we are to determine psychologically what we may expect from the individual.

While the replies to my letters showed that hardly any attention had so far been given to such problems of objective psychology in the industrial concerns, it became evident that the managers felt distinctly that here a problem was touched which must be of highest importance for economic success. From many different sides willingness was shown to study the problem of employment under the psychological aspect. As my material came mostly from very large establishments in which labor of very many different kinds is carried on side by side, of course I frequently received the assurance that whenever an industrious energetic man is unsuccessful in one kind of work, a trial is made with him in another department, and that by such shifting the right place can often be found for him. Young people,

to whom, in spite of long trial and the best will, it seems impossible to supply certain automatic machines, become excellent workers at much more difficult labor in the same establishment. Women who are apparently careless and inattentive when they have to distribute their attention over a number of operations do high-class work when they are engaged in a single activity; and in other cases the opposite is reported.

I may mention a few concrete chance illustrations. In a pencil factory the women in one department have to grasp with one movement a dozen pencils, no more and no less. Some learn this at once without effort, and they earn high wages; others never can learn it in spite of repeated trials. If those who fail in this department are transferred, for instance, to the department where the gold-leaf is most carefully to be applied to the pencils before stamping, very often they show great fitness in spite of the extreme exactitude needed for this work. To show how often activities which appear extremely similar may demand different individuals, if the work is based on different psychical functions, I may refer to a report from one of the largest establishments in the country. In the accounting department a large number of girls are occupied with looking over hundreds of thousands of slips from

which the weekly pay-list is compiled. Each slip contains six figures and small groups of twenty slips have to be looked through to see whether those six figures on each correspond. With moistened forefinger they turn up the slips one by one in much the same manner that a bank clerk counts money. A good sorter will turn up the slips so rapidly that a bystander is unable to read a single figure, and yet she will not overlook an error in thousands of slips. After the slips are sorted, the operation of obtaining the totals on each order number is performed with the aid of an adding machine. The machine operator rolls up the slips of the pile with the thumb of her left hand and transfers the amount to the proper keys of the machine. It has been found that the most rapid and accurate girls at sorting are not seldom useless on the machines. They press the wrong keys and make errors in copying the total from the machine indicators to the file-card. On the other hand, some of the best machine operators are very slow and inaccurate at the sorting table. Girls have been found very poor at the work at which they were first set, and very successful and efficient as soon as they had been transferred from the one to the other.

Examples of this kind might be heaped up without end. But while the very large establish-

ments demonstrate by such reports only that they can find somewhere a fit place for every able workingman if they take enough trouble to seek for it, after all the essential element of the reports remains, that successful achievement depends upon personal mental traits which cannot be acquired by mere good will and training. In view of this fact it is much more important that by far the majority of establishments have not such a great manifoldness of activities under one roof. The workingman who is a failure in the work which he undertook would usually have no opportunity to show his strong sides in the same factory, or at least to be protected against the consequences of his weak points. If his achievement is deficient in quality or quantity, he generally loses his place and makes a new trial in another factory under the same accidental conditions, without any deeper insight into his particular psychical traits and their relation to special industrial activities. But even in the large concerns, in which many kinds of labor are needed side by side, it is not the rule but a rare exception when the individual is systematically shifted to the psychologically correct place. A whole combination of conditions is necessary for that. If his mental unfitness makes him unsuccessful in one place, the position for which he is fit must

happen to be vacant. Moreover, he himself must like that other kind of work, and above all the foreman must recognize his particular fitness. In a few model factories in which the apprentice system is developed in the spirit of advanced sociological ideas, for instance, in the Lynn factory of the General Electric Company, such systematic efforts are being carried on and show fair results. But the regulation plan seems to be a haphazard lack of plan, and even the best endeavors probably fall short of what may be attained by the introduction of scientific psychological methods. So far in most factories the laborer who is not doing well simply loses his position, and by such an unfortunate experience he is not mentally enriched but impoverished, as he has lost much of his self-confidence and of his joy in labor.

If this limitless waste of human material, this pitiable crushing of joy in the day's work, and this crippling of the economic output is at last to be reduced, indeed nothing is more needed than a careful scrutiny of the various psychophysical functions involved in the work. A mere classification of the industrial occupations according to the classes of manufactured objects would be of no value for this need, as often a small industrial concern may embrace occupations which

are based on many different psychophysical functions. A harvester consists of two hundred and fifty different parts, and almost every one of these parts demands a long series of manufacturing processes. Thousands of different kinds of labor are thus combined in one factory and each process demands for the best work particular psychophysical traits, even though many of them can be carried out by quite unskilled laborers. In a large manufacturing establishment the manager assured me only recently that more than half a million different acts have to be performed in order to complete the goods of that factory. On the other hand, it evidently is proper to form larger groups in which processes are brought together which are similar with reference to the mental activity needed, while they may be dissimilar from the standpoint of industrial technique.

This analysis of the special processes can be furthered best by the coöperation of the experienced men of industry. Many of the replies which I received contained quite elaborated contributions to such a study of various industrial processes from a psychological point of view. They sometimes covered the ground from the simplest activity to the subtlest and most difficult economic tasks, and this, not only with reference to the functions of the laborer, but also even with

reference to the function of the industrial manager. The outsider can see these psychological requirements of the particular occupation only in crude outlines. The subtler nuances of differences between tasks can be gained only by an intimate knowledge of the industry. Again I may give an illustration. In the case of a well-known typesetting machine, thousands of which are in daily use, I had the impression that the rapidity of the performance was dependent upon the quickness of the finger reaction. The managers, on the other hand, have found that the most essential condition for speed in the whole work is the ability to retain a large number of words in memory before they are set. The man who presses the keys rather slowly advances more rapidly than another who moves his fingers quickly, but must make many pauses in order to find his place in the manuscript and to provide himself with new words.

The factors which are to be brought into correlation are, accordingly, first, the actual experiences of the managers, secondly, the observations of skilled psychologists in the industrial concerns, thirdly, psychological and experimental investigations with successful and unsuccessful laborers, and, fourthly, experimental studies of the normal variability. If such a programme is to be realized

in detail, it will be necessary to discriminate carefully between those mental traits of the personality which must be accredited to a lasting inherited disposition and such as have been developed under the influences of the surroundings, by education and training, by bad or good stimuli from the community. While those acquired traits may have become relatively lasting dispositions, their transformation is, after all, possible, and the limits in which changes may be expected will have to be found out by exact studies. Individual psychological rhythm, attention and emotion, memory and will energy, disposition to fatigue and to restoration, imagination, suggestibility and initiative, and many other features will have to be examined in their relation to the special economic aims.

Too much emphasis cannot be laid on another function as well, the experimental testing of which has only recently been started. I refer to the difference in the individual ability of men to profit from training. If we test an individual at a certain point in his life with regard to a variable ability, our result must be dependent upon three factors, the original disposition for the performance, the original disposition for the advance by training, and the training itself actually passed through up to that moment. A small amount of antecedent training for the particular task together with a

high ability to profit from repetition may be a better reason for the appointment of a man than a long training with small ability to profit from schooling, in spite of the fact that his actual achievement at this time may be in the first case smaller than in the second. He will do less at first, but he promises to outrank the other man after a period of further training. Special experiments must be carried on and have been actually started to determine this plasticity of the psychophysical apparatus as an independent inborn trait of the individual.[16]

This invasion of psychology into the field of economic activities is still so little advanced that the thought of a real distribution of the wage-earners among the various commercial and industrial positions on the basis of psychological tests would lead far beyond the present possibilities. Moreover, many factors would interfere with its being carried out consistently, even if a much higher stage of experimental research were reached. The thousands of social and local reasons which influence the choice of a vocation to-day would to a certain degree remain in force also in a period of better psychological analysis. Moreover, the personal inclinations and interests naturally would and ought to remain the mainspring of economic action. This inclination, which gives

so much of the joy in labor, is by no means necessarily coincident with those psychophysical dispositions which insure the most successful work. Political economists have found this out repeatedly from their statistical inquiries. Very careful studies of the textile industry in Germany carried out in recent years [17] yielded the result that the intelligent, highly trained textile laborer often dislikes his work the more, the more he shows ability for it, this ability being measured by the wages the individuals earn at piecework. The wage and the emotional attitude were not seldom inversely related. Those who were able to produce by far more than others and accordingly earned the most were sometimes the very ones who hated the work, while the less skillful workers earned less but enjoyed the work more. The consulting economic psychologist will, therefore, at first reasonably confine himself to warning the misfits at an early time. Even within these limits his service can be useful to both parties, the employers and the employees. He will only slowly reach the stage at which this negative warning may be supplemented by positive suggestions, as to the commercial industrial activities for which the psychophysical dispositions promise particular success.

A real assumption of responsibility for success

of course cannot be risked by the psychologist, inasmuch as the man who may be fitted for a task by his mental working dispositions may nevertheless destroy his chances for success by secondary personal traits. He may be dishonest, or dissipated, or a drinker, or a fighter, or physically ill. Finally, we ought not to forget that all such efforts to adjust to one another the psychological traits and the requirements of the work can never have reference to the extreme variations of human traits. The exceptionally talented man knows anyhow where he belongs, and the exceptionally untalented one will be excluded anyhow. The psychological aid in the selection of the fit refers only to the remaining four fifths of mankind for whom the chances of success can indeed be increased as soon as the psychological personal equation is systematically and with scientific exactitude brought into the calculation of the life development. How far a part of this effort will have to be undertaken by the school is a social problem which must be considered from various points of view. Its discussion would lead us beyond the limits of our present inquiry, but it seems probable that the real psychological laboratory experiment in the service of vocational guidance does not belong in the schoolroom itself, but ought to be left to special municipal institutions.

XII

INDIVIDUALS AND GROUPS

ONE point here must not be overlooked. The effort to discover the personal structure of the individual in the interest of his vocational chance does not always necessarily involve a direct analysis of his individuality, as material of some value can be gained indirectly. Such indirect knowledge of a man's mental traits may be secured first of all through referring the man to the groups to which he belongs and inquiring into the characteristic traits of those groups. The psychology of human variations gives not only an account of the differences from person to person, but studies no less the psychical inequalities of the races, of the nations, of the ages, of the professions, and so on. If an economic activity demands a combination of mental traits, we may take it for granted that an individual will be fit for the work as soon as we find out that he belongs to a group in which these required mental traits habitually occur. Such a judgment based on group psychology can of course be no more than a mere approach to a solution of the problem, as the psychical qualities may vary strongly in the midst of the group. The special individual may happen to

stand at the extreme limit of the group, and the traits which are usually characteristic of it may be very little developed or entirely lacking in his special case. We may know that the inhabitants of a special country are rather alert, and yet the particular individual with whom we have to deal may be clumsy and phlegmatic. The interests of economy will, therefore, be served by such considerations of group psychology only if the employment, not of a single person, but of a large number, is in question, as it is most probable that the average character will show itself in a sufficient degree as soon as many members of the group are involved.

Even in this case the presupposition ought to be that the average characteristics are found out with scientific exactitude by statistical and experimental methods, and not that they are simply deduced from superficial impressions. I have found that just this race psychological diagnosis is frequently made in factories with great superficiality. Some of the American industrial centres offer extremely favorable conditions for the comparative study of nationality. I have visited many manufacturing establishments in which almost all workers are immigrants from foreign countries and in which up to twenty different nationalities are represented. The employment officers there easily develop some psychological

theories on the basis of which they are convinced that they are selecting the men with especial skill, knowing for each in which department he will be most successful. They consider it settled that for a particular kind of activity the Italians are the best, and for another, the Irish, and for a third, the Hungarians, and for a fourth, the Russian Jews. But as soon as these factory secrets have been revealed, you may be surprised to find that in the next factory a decidedly different classification of the wage-earners is in force. In a gigantic manufacturing concern, I received the definite information that the Swedish laborers are preferable wherever a steady eye is needed, and in another large factory on the same street I was assured that just the Swedes are unfit for such work. Sometimes this diversity of opinion is the result of different points of view. In one factory in which a certain industrial operation is rather dangerous, they told me that they took no southern Europeans, especially no Italians and Greeks, because they are too hasty and careless in their movements, while they gladly filled the places with Irishmen. In a quite similar factory, on the other hand, they had a prejudice against the Irishmen alone for this work, because the Irish laborers are too willing to run a risk and to expose themselves to danger. Probably both psycho-

logical observations are on the whole correct, but in the first factory only the one and in the second factory only the other was recognized. Much more thorough statistical inquiries than those which as yet exist, especially as to the actual differences of wages and piecework for wage-earners of various nationalities, would have to furnish a basis for such race psychological statements, until the time arrives when the psychological experiment comes to its own.

In a similar way so far we have to rely on general theories of group psychology when the psychological differences of the sexes are to be reckoned with in economic interests. So long as laboratory methods for individual tests are not usual, the mental analysis of the general groups of men and women must form the background for industrial decisions. To be sure, it is not difficult to emphasize certain mental traits as characteristic of women in general in contrast to men in general, and to relate them to certain fundamental tendencies of their psychophysical organism. As soon as this is done, it is easy theoretically to deduce that certain industrial functions are excellently adapted to the minds of women and that certain others stand in striking antagonism to them. If the employment of large numbers is in question, and average values alone are involved, such a de-

cision on the basis of group psychology may be adequate. In most factories this vague sex psychology, to be sure, usually with a strong admixture of wage questions, suggests for which machines men and for which women ought to be employed. But here again it is not at all improbable that in the case of a particular woman the traditional group value may be entirely misleading and the personality accordingly unfit for the place. Only the subtle psychological individual analysis can overcome the superficial prejudices of group psychology. The situation lies differently when problems of economic policy are before us. Such general policies as, for instance, colonial politics, or immigration politics, or politics concerned with city and rural communities, or with coast and mountain population, will always have to be based on group psychology as far as the economic problems are involved, inasmuch as they refer to the average and not to the individual differences.

Finally, another indirect scheme to determine the personal qualities needed for economic efficiency may be suggested by the psychology of the typical correlations of human traits. We have seen that group psychology proclaims that a certain individual probably has certain traits because he belongs to this or that nation or to this

or that otherwise well-known group. Correlation psychology proclaims that a particular individual possesses or does not possess certain traits because he shows or does not show some other definite qualities. A correlation, for instance, which the commercial world often presupposes, may exist between individual traits and the hand-writing. Graphologists are convinced that a certain loop or flourish, or the steepness or the length of the letters, or the position of the *i* dot, is a definite indication that the writer possesses certain qualities of personality; and if just these qualities are essential requirements for the position, the impression of the handwriting in a letter may be taken as a sufficient basis for appointment. The scientist has reason to look upon this particular case of graphological correlation with distrust. Yet even he may acknowledge that certain correlations exist between the neatness, carefulness, uniformity, energy, and similar features of the letter, and the general carefulness, steadiness, neatness, and energy of the personality.

However, the laboratory psychologists nowadays have gone far beyond such superficial claims for correlations of symptoms. With experimental and statistical methods they have gathered ample material which demonstrates the exact degree of probability with which we have a right to expect

that certain qualities will occur together. Theoretically we may take it for granted that those traits which are always present together or absent together ultimately have a common mental root. Yet practically they appear as two independent traits, and therefore it remains important to know that, if we can find one of them, we may be sure that the other will exist there too. Inasmuch as the one of the two traits may be easily detected, while the other may be hidden and can be found out only by long careful tests, it would be valuable, indeed, for the employment manager to become acquainted with such correlations as the psychologist may discover: as soon as he becomes aware of the superficially noticeable symptom, he can foresee that the other disposition is most probably present. To give an illustration: in the interest of such measurements of correlations we have studied in the Harvard laboratory the various characteristics of attention and their mutual dependence.[18] We found that typical connections exist between apparently independent features of attention. Persons who have a rather expansive span of attention for acoustical impressions have also a wide span for the visual objects. Persons whose attention is vivid and quick have on the whole the expansive type of attention, while those who attend slowly have a

narrow field of attention, and so on. Hence the manifestation of one feature of attention allows us to presuppose without further tests that certain other features may be expected in the particular individual.

The problem of attention, indeed, seems to stand quite in the centre of the field of industrial efficiency. This conviction has grown upon me in my observation of industrial life. The peculiar kind of attention decides more than any mental trait for which economic activity the individual is adapted. The essential point is that such differences of attention cannot be characterized as good or bad; it is not a question of the attentive and of the inattentive mind. One type is not better than another, but is simply different. Two workingmen, not only equally industrious and capable, but also equally attentive, may yet occupy two positions in which they are both complete failures because their attention does not fit the places, and both may become highly efficient as soon as they exchange positions. Their particular types of attention have now found the right places. The one may be disposed to a strong concentration by which everything is inhibited which lies on the mental periphery, the other may have the talent for distributing his attention over a large field, while he is unable to hold it for a long

while at one point. If the one industrial activity demands the attentive observation of one little lever or one wheel at one point, while the other demands that half a dozen large machines be simultaneously supervised, all that is necessary is to find the man with the right type of attention for each place. It would be utterly arbitrary to claim that the expansive type of attention is economically more or less valuable than the concentrated type. Both in English and in German we have a long popular series of pamphlets with descriptions of the requirements and conditions for the various occupations to which a boy or a girl may turn, but I have nowhere found any reference to the most essential mental functions such as the particular kind of attention or memory or will. These pamphlets are always cut after the same pattern. Where the detail refers at all to the mental side, it points only to particular knowledge which may be learned in school or trade or work, or to abilities which may be developed by training. But the individual differences which are set by the particular conditions and dispositions of the mind are neglected with surprising uniformity in the vocational literature of all countries. The time seems ripe for at last filling this blank in the consciousness of the nation and in the institutions of the land.

PART II

THE BEST POSSIBLE WORK

PART II

THE BEST POSSIBLE WORK

XIII

LEARNING AND TRAINING

WE have placed our psychotechnical interest at the service of economic tasks. We therefore had to start from the various economic purposes and had to look backward, asking what ways might lead to these goals. All our studies so far were in this sense subordinated to the one task which ought to be the primary one in the economic world, and yet which has been most ignored. The purpose before us was to find for every economic occupation the best-fitted personality, both in the interest of economic success and in the interest of personal development. Individual traits under this point of view become for us the decisive psychological factors, and experimental psychology had to show us a method to determine those personal differences and their relation to the demands for industrial efficiency. This first goal may be reached with all the means of science, as we hope it will be in the future, or

everything may be left to unscientific, haphazard methods as in the past: in any case a second task stands before the community, namely, the securing of the best possible work from every man in his place. Indeed, the nation cannot delay the solution of this second problem until the first has been solved in a satisfactory way. We might even say that the answer to the second question is the more important, the less satisfactory the answer to the first is. If every place in the economic world were filled only by those who are perfectly adapted by their mental traits, it would be much less difficult to get efficient work from every one. The fact that so many misfits are at work makes it such an urgent necessity to find ways and means by which the efficiency can be heightened.

It must be acknowledged, however, that the problem of the best work is not quite such a clear one as that of the best man. From various standpoints a different answer may be given to the question which kind of work is the best. A capitalistic, profit-seeking egotism may consider the quickest performance, or, if differences of quality are involved, the most skillful performance, the only desirable end. The social reformers, on the other hand, may consider the best work that which combines the greatest and best possible output with the highest possible saving of the organism

and the fullest development of the personality. We have emphasized from the start that the practical psychologist as such has not the right to give a decision upon problems of social civilization. He has to accept the economic tasks from the community for which he is working and his impartial service commences only when the goals have been determined. It is not his share to select the ends, but simply to determine the means after the valuable ends have been chosen. As a psychological scientist he has not the right to enter into the arena of different social party fights. Yet we find after all a broad region which seems rather untouched by any conflict of reasonable opinions. A reckless capitalism on the one side and a feeble sentimentality on the other side may try to widen or to narrow the boundaries of this region, but taken all together, a vigorous healthy nation which is eagerly devoted to its work is on the whole in agreement as to the essential economic demands for efficient labor.

Experience, to be sure, shows that great changes in the conditions of work can never enter into the history of civilization without certain disturbances, and that opposition must therefore necessarily arise in certain groups even against such changes as are undoubtedly improvements and advances from the point of view of the whole na-

tion. Such dissatisfaction arose when the factory system was introduced, and it is only natural that some irritation should accompany the introduction of psychological improvements in the methods of work, inasmuch as not a few wage-earners may at first have to lose their places because a small number of men will under the improved conditions be sufficient for the performance of tasks which needed many before. But the history of economics has clearly shown that from the point of view of the whole community such an apparent disturbance has always been only temporary. If the psychologists succeed in fundamentally improving the conditions of labor, the increased efficiency of the individual will promote such an enriched and vivified economic life that ultimately an increase in the number of laborers needed will result. The inquiry into the possible psychological contributions to the question of reinforced achievement must not be deterred by the superficial objection that in one or another industrial concern a dismissal of wage-earners might at first result. Psychotechnics does not stand in the service of a party, but exclusively in the service of civilization.

To begin at the beginning, we may start from the commonplace that every form of economic labor in the workshop and in the factory, in the field and in the mine, in the store and in the office,

must first be learned. How far do the experiments of the psychologist offer suggestions for securing the most economic method of learning practical activities? Bodily actions in the service of economic work are taught and learned in hundreds of thousands of places. It is evident that one method of teaching must reach the goal more quickly and more reliably than another. Some methods of teaching must therefore be economically more advantageous, and yet on the whole the methods of teaching muscular work are essentially left to chance. It is indeed not difficult to observe how factory workers or artisans have learned the same complex motion according to entirely different methods. The result is that they carry out the various partial movements in a different order, or with different auxiliary motions, or in different positions, or in a different rhythm, or with different emphasis, simply because they imitate different teachers, and because no norm, no certainty as to the best methods for the teaching, has been determined. But the process of learning is still more fluctuating and still more dependent upon chance than the process of teaching. The apprentice approaches the instruction in any chance way, and the beginner usually learns even the first steps with a psychophysical attitude which is left to accident. An immense waste of

energy and a quite anti-economic training in unfit movements is the necessary result.

The learning of the elements of school knowledge in the classroom in earlier times proceeded after exactly such chance methods. Any one who knew how to read, write, and calculate felt himself prepared to pour reading, writing, and arithmetic into the unprotected children. Methods which are based on scientific examination of the psychophysical process of reading and writing were not at the disposal of the schools, and exact results from comparative studies of pedagogical methods had not been secured. The last few decades have created an entirely new foundation for enlightened school work. The experimental investigations of pedagogical psychology have determined exactly how the consciousness of the child reacts on the various methods of teaching and have built up a real systematic economic learning. All which was left to dilettantic caprice has been transformed into more or less definite standard forms. For instance, the old scheme of teaching reading by the alphabet method is practically eliminated from our modern schools. It is clear that this learning of the names of the single letters as a starting-point for the reading of words was not only a wasting of time and energy, but an actual disturbance in the

development of the reading process in the older generation. As those names of the letters do not occur at all in the words to be read, but only their sounds, what had been learned in seeing the single letters had to be inhibited in pronouncing the whole word. It seems not too much to say that the learning of industrial activities on the whole still stands on the level of such alphabet methods, and this cannot be otherwise, as the real problem, namely, the systematic investigation of the psychophysical activities involved, has never been brought into the psychological laboratory.

The pedagogical experiment has shown clearly enough that the subjective feeling of easier or quicker learning may be entirely unreliable and misleading. If the task is to learn a page by heart, we may proceed after many different methods. We may learn very small fractions of the text, repeating only a few words, or we may read whole paragraphs every time; we may repeat the whole material again and again, or we may put in long periods of rest after a few repetitions; we may frequently recite it from memory and have some one to prompt us; we may give our attention especially to the meaning of the words, or merely to the sounds, or we may introduce any number of similar variations. Now the careful experiment shows that of two such methods one which

appears to us the better and more appropriate in learning, perhaps even as the easier and more comfortable, may prove itself the less efficient one in the practical result. The psychology of learning, which won its success by introducing meaningless syllables as experimental material, has slowly determined the most reliable methods for impressing knowledge on memory. Where such results have once been secured, it would surely be a grave mistake simply to stick to the methods of so-called common sense and to leave it to the caprice of the individual teacher to decide what method of learning he will suggest to his pupils. The best method is always the only one which should be considered. The psychology of economic work must aim toward similar goals. We must secure a definite knowledge as to the methods by which a group of movements can best be learned. We must understand what value is to be attached to the repetitions and to the pauses, to the imitations and to the special combinations of movements, to the exercise in parts of the movements, to the rhythm of the work, and to many similar influences which may shape the learning process.

The simplest aspect, that of the mere repetition of the movement, has frequently been examined by psychophysicists. The real founder of experimental psychology, Fechner, showed the

way; he performed fatiguing experiments with lifted dumb-bells. Then came the time in which the laboratories began to make a record of the muscular activities with the help of the ergograph, an instrument with which the movements of the arm and the fingers can easily be registered on the smoked surface of a revolving drum. The subtlest variations of the activity, the increase and decrease of the psychomotor impulse, the mental fatigue, can be traced exactly in such graphic records. This psychomotor side of the process, and not the mere muscle activity as such, is indeed the essential factor which should interest us. The results of exercise are a training of the central apparatus of the brain and not of the muscular periphery. The further development of those experiments soon led to complex questions, which referred not only to the mere change in the motor efficiency, but to the learning of particular groups of movements and to the influences on the exactitude and reliability of the movements. The purely mental factors of the will-impulse, especially the consciousness of the task, came into the foreground. These experiences of the scientists concerning the influences of training, the mechanization of repetition, and the automatization of movements have been thoroughly discussed by a brilliant political economist [19] as an explana-

tion of certain industrial facts, but they have not yet practically influenced life in the factory.

The nearest approach from the experimental side to the study of the effect of training in actual industrial tasks may be found in certain laboratory investigations which refer to the learning of telegraphing, typewriting, and so on. For instance, we have a careful study [20] of the progress made in learning telegraphy, both as to the transmitting of the telegrams by the key movement and the receiving of the telegrams by the ear. It was found that the rapidity of transmitting increases more rapidly and more uniformly than the rapidity of receiving. But while the curve of the latter rises more slowly and more irregularly, it finally reaches the greater height. The ability in transmitting, represented by a graphic record, shows an ascent which corresponds to the typical, steady curve of training. In the receiving curve, on the other hand, we find not far from the beginning a characteristic period during which no progress whatever can be noticed, and this is also repeated at a later stage. The psychological analysis shows that the increase of ability in the receiving of telegrams depends upon the development of a complex system of psychophysical habits. The periods in which the curve does not ascend represent stages of

training in which the elementary habits are almost completely formed, but have not become sufficiently automatic. The attention is therefore not yet ready to start habits of a higher order. The lowest correlation refers to the single letters, after that to the syllables and words. As soon as the apprentice has reached this point, he stops, because he must learn to master more and more new words until his telegraphic vocabulary is large enough to make it possible for him to turn his consciousness to whole groups of words at once. Only when this new habit has been made automatic by a training of several months can he advance to a level at which whole groups of words are perceived as telegraphic units. A time follows in which this mastery of whole phrases advances rapidly, until a new period of rest comes, from which, only after years and often quite suddenly, a last new ascent can be noticed. Instead of concentrating the attention with conscious strain on single phrases, the operator progresses to a perfect liberty in which whole sentences are understood automatically.

We also have a model experimental research into the psychological conditions of learning in the case of writing on a typewriter.[21] By electrical connections between the typewriting machine and a system of levers which registered their

movements on the rotating drum of a kymograph, each striking of a key, each completion of a word, or of a line, could be recorded in exact time-relations. Each glance at the copy was also registered. It was found that the process of learning consisted first of a continuous simplification of the cumbersome methods with which the beginner commences. A steady elimination of unfit movements, a selection, a reorganization, and finally, a combination of psychophysical acts to impulses of higher order, could be traced exactly. Here, too, the curve of learning at first rises quickly and then more and more slowly. Of course the usual fluctuations in the growth of the ability can also be found, and above all the irregular periods of rest in which the learning itself does not progress, for some of these so-called plateaus which lie between the end of one ascent and the beginning of the next may cover a month and more. At the beginning we have the elementary association between the single letter and the position of the corresponding key, but soon an immediate connection between the visual impression of the whole syllable or the whole word and the total group of movements necessary to strike the keys for it is developed. The more the ability grows, the more these psychical impulses of higher order become organized without conscious intention.

The study shows that this development of higher habits has already begun before the lower habits are fully settled.

How far the special training involves at the same time a general training which could be of advantage for other kinds of labor has not yet been studied at all with reference to industrial technique. There we are still completely dependent upon certain experiences in the field of experimental pedagogy, and upon certain statistics, for instance, in the textile industry. Many patient investigations, with every independent group of apparatus and machines, may be necessary before psychotechnics will be able to supply industry with reliable advice for teaching and learning. Nor have we the least right hastily to carry over the results from one group of movements to another. Even where superficially a certain similarity between the technical factors exists, the psychophysical conditions may be essentially different. In the two cases mentioned, for instance, telegraphing and typewriting, the chief factor seems the same, as in both cases the aim is to make the quickest possible finger movements for purposes of signals; and yet it is not surprising that the development of the ability from the beginnings to the highest mastery is rather unlike, as all the movements in telegraphing are per-

formed with the same finger, while in typewriting the chief trait is the organization of groups from the impulses to all ten fingers. At least it is certain that learning always means far more than a mere facilitation of the movement by mechanical repetition, and this is true of the simplest handling of the tools in the workshop, of the movements at the machine in the factory, and of the most complex performances at the subtlest instruments. The chief factor in the development is always the organization of the impulses by which the reactions which are at first complicated become simplified, later mechanized, and finally synthesized into a higher group which becomes subordinated to one simple psychical impulse. The most reliable and psychophysically most economic means for this organization will have to be studied in the economic psychological laboratories of the future for every particular technique. Then only can the enormous waste of psychical energy resulting from haphazard methods be brought to an end.

A problem which is still too little considered in industrial life is the mutual interference of acquired technical activities. If one connected series of movements is well trained by practice, does it become less firmly fixed, if another series is studied in which the same beginning is connected with

another path of discharge? I approached this psychophysical question of learning by experiments which I carried on for a long while with variations of ordinary habits of daily life, asking whether a habit associated with a certain sensory stimulus can function automatically while dispositions for a different habit, previously acquired, remain in the psychophysical system. For instance, I was accustomed to carry my watch in my left-hand vest pocket. For a week I carried it in the right-hand pocket of my trousers and recorded every case in which I first automatically made the movement to the vest. After some time the movement to the right-hand pocket became entirely automatic. When it was sufficiently fixed, I again put the watch in the left-hand vest pocket and recorded how often I unconsciously grasped at the right side when I wanted to see what time it was. As soon as the vest pocket movement had again become fixed, I went back to the right-hand trousers pocket. And so I alternated for a long while, always changing only after reaching complete automatism. But the results in this case and in other similar experiments which I carried on showed that the new automatic connection did not extinguish the after effects of the previous habit. With every new change the number of wrong movements became smaller and smaller,

and finally a point was reached at which the dispositions for both movements were equally developed so that no wrong movements occurred when the watch was put into the new position.[22]

This problem has been followed up very recently in a valuable investigation at Columbia University,[23] in which various habits of typewriting and of card-sorting were acquired and studied in their mutual interference. These very careful experiments also show that when two opposing associations are alternately practiced, they have an interference effect on each other, but that the interference grows less and less as the practice effect becomes greater. The interference effect is gradually overcome and both opposing associations become automatic, so that either of them can be called up independently without the appearance of the other. Many details of the research suggest that this whole group of interference problems deserves the most careful attention by those who would practically profit from increased industrial efficiency.

Finally, in the experimental study of the problem of technical learning, we cannot ignore the many side influences which may hasten or delay, improve or disturb, the acquisition of industrial skill. In the Harvard laboratory, for instance, we

are at present engaged in an investigation which deals with the influence of feelings on the rapidity with which new movement coördinations are mastered.[24] In order to have unlimited comparable material a very simple technical performance is required, namely, the distribution of the 52 playing-cards into 52 boxes. Labels on the boxes indicate changing combinations for the distribution to be learned. We examine, on the one side, the influence of feelings of comfort or of discomfort on the learning of the new habit, these feeling states being produced by external conditions, such as pleasant or unpleasant sounds, odors, and so on. On the other side we trace the effects of those feelings which arise during the learning process itself, such as feelings of satisfaction with progress, or disappointment, or discomfort, or disgust or joy in the activity.

XIV

THE ADJUSTMENT OF TECHNICAL TO PSYCHICAL CONDITIONS

TEACHING and learning represent only the preliminary problem. The fundamental question remains, after all, how the work is to be done by those who have learned it in accordance with the customs of the economic surroundings and who are accordingly already educated and trained for it. What can be done to eliminate everything which diminishes and decreases efficiency, and what remains to be done to reinforce it. Such influences are evidently exerted by the external technical conditions, by variations of the activity itself, and by the play of the psychical motives and counter-motives. It must seem as if only this last factor would belong in the realm of psychology, but the technical conditions, of which the machine itself is the most important part, and the bodily movements also have manifold relations to the psychical life. Only as far as these relations prevail has the psychologist any reason to study the problem. The purely physical and economic factors of technique do not interest him at all, but when a technical arrangement makes a psychophy-

sical achievement more difficult or more easy, it belongs in the sphere of the psychologist, and just this aspect of the work may become of greatest importance for the total result. In all three of these directions, that is, with reference to the technical, to the physiological, and to the purely psychical, the scientific management movement has prepared the way. The engineers of scientific management recognized, at least, that no part of the industrial process is indifferent; even the apparently most trivial activity, the slightest movement of arm or hand or leg, became the object of their exact measurement. The stopwatch which measures every movement in fractions of a second has become the symbol of this new economic period. As long as special psychological experiments in the service of industrial psychology are still so exceptional, it may, indeed, be acknowledged that the practical experiments in the service of scientific management have come nearest to the solution of these special psychotechnical problems.

To proceed from without toward the centre, we may begin our review with the physical technique of the working conditions and its relations to the mind. The history of technique shows on every page this practical adjustment of external labor conditions to the psychophysical necessities

and psychophysical demands. No machine with which a human being is to work can survive in the struggle for technical existence, unless it is to a certain degree adapted to the human nerve and muscle system and to man's possibilities of perception, of attention, of memory, of feeling, and of will. Industrial technique with its restless improvements has always been subordinated to this postulate. Every change which made it possible for the workingman to secure equal effects with smaller effort or to secure greater or better effects with equal effort counted as an economic gain, which was welcome to the market. For instance, throughout the history of industry we find the fundamental tendency to transpose all activities from the great muscles to the small muscles. Any activity which is performed with the robust muscles of the shoulder when it can be done with the lower arm, or labor which is demanded from the muscles of the lower arm when it can just as well be carried out by the fingers, certainly involves a waste of psychophysical energy. A stronger psychophysical excitement is necessary in order to secure the innervation of the big muscles in the central nervous system. This difference in the stimulation of the various muscle groups has been of significant consequence for the differentiation of work throughout the development of mankind.[25]

Labor with the large muscles has, for these psychophysical reasons, never been easily combined with the subtler training of the finer muscles. Hence a social organization which obliged the men to give their energy to war and the hunt, both, in primitive life, functions of the strongest muscles, made it necessary for the domestic activities, which are essentially functions of the small muscles, to be carried out by women. The whole history of the machine demonstrates this economic tendency to make activities dependent upon those muscles which presuppose the smallest psychophysical effort. It is not only the smaller effort which gives economic advantage to the stimulation of the smaller muscles, but the no less important circumstance that the psychophysical after-effect of their central excitement exerts less inhibition than the after-effect of the brain excitement for the big muscles.

But we must not overlook another feature in the development of technique. The machines have been constantly transformed in the direction which made it possible to secure the greatest help from the natural coördination of bodily movements. The physiological organization and the psychophysical conditions of the nervous system make it necessary that the movement impulses flow over into motor side channels and thus

produce accessory effects without any special effort. If a machine is so constructed that these natural accessory movements must be artificially and intentionally suppressed, it means, on the one side, a waste of available psychophysical energy, and on the other side it demands a useless effort in order to secure this inhibition. The industrial development has moved toward both the fructification of those side impulses and the avoidance of these inhibitions. It has adjusted itself practically to the natural psychical conditions. Ultimately it is this tendency which shaped the technical apparatus for the economic work until the muscle movements could become rhythmical. The rhythmical activity necessarily involves a psychophysical saving and this saving has been instinctively secured throughout the history of civilization. All rhythm contains a repetition of movement without making a real repetition of the psychophysical impulse necessary. In the rhythmical activity a large part of the first excitement still serves for the second, and the second for the third. Inhibitions fall away and the mere after-effect of each stimulus secures a great saving for the new impulse. The history of the machine even indicates that the newer technical development not only found the far-reaching division of labor already in the workshops of

earlier centuries, but a no less far-reaching rhythmization of the labor in fine adaptation to the needs of the psychophysical organism, long before the appearance of the machines. The beginnings of the machine period frequently showed nothing but an imitation of the rhythmical movements of man.[26] To be sure, the later improvements of the machine have frequently destroyed that original rhythm of man's movement, as the movement itself, especially in the electric machines, has become so quick that the subjective rhythmical experience has been lost. Moreover, the rhythmical horizontal and vertical movements were for physical reasons usually replaced by uniform circular movements. But even the most highly developed machine demands human activity, for instance, for the supplying with material; and this again has opened new possibilities for the adjustment of technical mechanism to the economic demand for rhythmical muscle activity. The growth of technical devices has thus been constantly under the control of psychological demands, in spite of the absence of systematic psychological investigations. But the decisive factor was, indeed, that these psychological motives always remained in the subconsciousness of civilization. The improvements were consciously referred to the machine as such, however much the

practical success was really influenced by the degree of its adjustment to the mental conditions of the workingmen. The new movements of scientific management and of experimental psychology aim toward bringing this adaptation consciously into the foreground and toward testing and studying systematically what technical variations can best suit the psychophysical status of man.

Those who are familiar with the achievements of scientific management remember that by no means only the complicated procedures on a high level are in question. The successes are often the most surprising where the technique is old, and where it might have been imagined that the experiences of many centuries would have secured through mere common sense the most effective performance. The best-known case is perhaps that of the masons, which one of the leaders of the scientific management movement has studied in all its details.[27] The movements of the builders and the tools which they use were examined with scientific exactitude and slowly reshaped under the point of view of psychology and physiology. The total result was that after the new method 30 masons completed without greater fatigue what after the old methods it would have taken 100 masons to do, and that the total ex-

pense for the building was reduced to less than a half in spite of the steady increase of the wages of the laborers. For this purpose it was necessary that exact measurements be made of the height at which the bricks were lying and of the height of the wall on which they must be laid, and of the number of bricks which should be carried to the masons at once. He studied how the trowel should be shaped and how the mortar should be used and how the bricks should be carried to the bricklayers. In short, everything which usually is left to tradition, to caprice, and to an economy which looks out only for the most immediate saving, was on the basis of experiments of many years replaced by entirely new means and tools, where nothing was left to arbitrariness. Yet these changes did not demand any invention or physically or economically new ideas, but merely a more careful adaptation of the apparatus to the psychological energies of the masons. The new arrangement permitted a better organization of the necessary bodily movements, fatigue was diminished, the accessory movements were better fructified, fewer inhibitions were necessary, a better playing together of the psychical energies was secured.

The students of scientific management stepped still lower in the scale of economic activity. There

is no more ordinary productive function than shoveling. Yet in great establishments the shoveling of coal or of dirt may represent an economically very important factor. It seems that up to the days of scientific management, no one really looked carefully into the technical conditions under which the greatest possible economic effect might be reached. Now the act of shoveling was approached with the carefulness with which a scholar turns to any subtle process in his laboratory. The brilliant originator of the scientific management movement, who carried out these investigations [28] in the great Bethlehem Steel Works, where hundreds of laborers had to shovel heavy iron ore or light ashes, found that the usual chance methods involve an absurd economic waste. The burden was sometimes so heavy that rapid fatigue developed and the movements became too slow, or the lifted mass was so light that the larger part of the laborer's energies remained unused. In either case the final result of the day's work must be anti-economic. He therefore tested with carefully graded experiments what weight ensured the most favorable achievement by a strong healthy workingman. The aim was to find the weight which would secure with well-arranged pauses the maximum product in one day without over-fatigue. As soon as this weight was deter-

mined, a special set of shovels had to be constructed for every particular kind of material. The laborers were now obliged to operate with 10 different kinds of shovels, each of such a size that the burden always remained an average of 21 pounds for any kind of material. The following step was an exact determination of the most favorable rapidity and the most perfect movement of shoveling, the best distribution of pauses, and so on, and the final outcome was that only 140 men were needed where on the basis of the old plan about 500 laborers had been engaged. The average workingman who had previously shoveled 16 tons of material, now managed 59 tons without greater fatigue. The wages were raised by two thirds and the expenses for shoveling a ton of material were decreased one half. This calculation of expenses included, of course, a consideration of the increased cost for tools and for the salaries of the scientific managers.

Whoever visits factories in which the new system has been introduced by real specialists must be surprised, indeed, by the great effects which often result from the better psychophysical adaptation of the simplest and apparently most indifferent tools and means. As far as the complicated machines are concerned, we are accustomed to a steady improvement by the efforts of the

technicians and we notice it rather little if the changes in them are introduced for psychological instead of the usual physical reasons. But the fact that even the least complicated and most indifferent devices can undergo most influential improvements, as soon as they are seriously studied from a psychological point of view, remains really a source for surprise. Sometimes no more is needed than a change in the windows or in the electric lamps, by which the light can fall on the work in a psychologically satisfactory way; sometimes long series of experiments have to be made with a simple hammer or knife or table. Often everything must be arranged against the wishes of the workingmen, who feel any deviation from the accustomed conditions as a disturbance which is to be regarded with suspicion. In one concern I heard that the scientific manager became convinced that all the working-chairs for the women were too low and that the laborers therefore had to hold their arms in a psychophysically unfavorable position during the handling of the apparatus. All were strongly opposed to the introduction of higher chairs. The result was that the manager arranged for the chairs to be raised a few millimeters every evening, without the knowledge of the working-women, as soon as the factory was empty. After a few weeks the chairs had reached

the right height without those engaged in the work having noticed it at all. The outcome was a decided increase of efficiency.

But the most rational scheme will after all be to prepare for such arrangements of tools and apparatus by systematic experiments in the psychological laboratory. The subtlety of such investigations will lead far beyond the point which is accessible to the attempts of scientific management. Exact experiments on attention, for instance, will have to determine how the various parts of the apparatus are to be distributed best in space if the laborer must keep watch for disturbances at various places. Only the laboratory experiment can find the most favorable speed of the machine or can select the muscles to which the mind can send the most effective impulses. The construction of the machine must then be adapted to such results. In the Harvard laboratory, for instance, a practical question led us to examine which fingers would allow the quickest alternation of key movements.[29] If any two of the ten fingers perform for ten seconds the quickest possible alternation of motion, as in a trill, the experiment can demonstrate exactly the differences between the various combinations of fingers and the individual fluctuations for these differences. With an electrical

registration of the movements of the alternating fingers we studied in hundredths of a second the time for the motions of two hands and of fingers of the same hand, in order to adjust the keys of a certain machine to the most favorable impulses.

We approach this group of problems from another side when we test the relations of various kinds of machines to various mental types. Psychologists have studied, for example, the various styles of typewriting machines.[30] From a purely commercial point of view the merits of one or another machine are praised as if they were advantageous for every possible human being. The fact is that such advantages for one may be disadvantages for another on account of differences in the mental disposition. One man may write more quickly on one, another on another machine. As every one knows, the chief difference is that of the keyboard and that of the visible or invisible writing. Machines like the Remington machine work with a shift key; that is, a special key must be pressed when capital letters are to be written. Other machines like the Oliver even demand double shifting, one key for the capital letters, and one for the figures, and so on. On the other hand, machines like the Smith Premier have no shift key, but a double keyboard.

It is evident that both the shift-key arrangement and the double keyboard have their particular psychological advantages.

The single alphabet demands much less from the optical memory, and the corresponding motor inner attitude of consciousness is adjusted to a smaller number of possibilities. But the pressure on the shift key, which goes with the single alphabet, is not only a time-wasting act; from the psychological point of view it is first of all a very strong interruption of the uniform chain of impulses. If the capital and small letters are written for a minute alternatingly with the greatest possible speed, the experiment shows that the number of letters for the machine with the double alphabet is about three times greater than for the machine with simple alphabet and shift key. Both systems accordingly have their psychological advantages and disadvantages. Human beings of distinct visual ideational type or of highly developed motor type will prefer the double alphabet, provided, of course, that the touch system of writing is learned, and this will be especially true if their inner attitude is easily disturbed by interruptions. But those who have a feebly developed optical mental centre and who have small ability for the development of complex motor habits will be more efficient on the ma-

chines with the single alphabet, especially if their nervous system is little molested by interruptions and thus undisturbed by the intrusion of the shift key act.

In a similar way the visibility of the writing will be for certain individuals the most valuable condition for quick writing, while for others, who depend less upon visual support, it may mean rather a distraction and an interference with the speediest work. The visible writing attracts the involuntary attention, and thus forces consciousness to stick to that which has been written instead of being concentrated on that which is to be produced by the next writing movements. The operator himself is not aware of this hindrance. On the contrary, the public will always be inclined to prefer the typewriters with visible writing, because by a natural confusion the feeling arises that the production of the letter is somewhat facilitated, when the eye is coöperating, just as in writing with a pen we follow the lines of the written letter. But the situation lies differently in the two cases. When we are writing with a pen, the letter grows under our eyes, while in the machine writing we do not see any part of the letter until the whole movement which produces the single letter is finished. By such a misleading analogy many a man is led to prefer the

typewriter with visible writing, while he would probably secure a greater speed with a machine which does not tempt him to attend the completed letters, while his entire attention ought to belong to the following letters.

These last observations point to another psychological aspect of the machine and of the whole technical work, namely, their relations to the impressions of the senses. The so-called dynamogenic experiments of the psychological laboratory have demonstrated what a manifold influence flows from the sense-impressions to the will-impulses. If the muscle contraction of a man's fist is measured, the experiment shows that the strongest possible pressure may be very different when the visual field appears in different colors, or tones of different pitch or different noises are stimulating the ear, and so on. As yet no systematic experiments exist by which such results can be brought into relation to the sense-stimuli which reach the laborer during his technical work. The psychophysical effect of colors and noises has not been fructified at all for industrial purposes. The mere subjective judgment of the workingman himself cannot be acknowledged as reliable in such questions. The laborer, for instance, usually believes that a noise to which he has become accustomed does not disturb him in his work,

while experimental results point strongly to the contrary. In a similar way the effect of colored windows may appear indifferent to the workmen, and yet may have considerable influence on his efficiency. Numberless performances in the factory are reactions on certain optical or acoustical or tactual signals. Both the engineer and the workman are satisfied if such a signal is clearly perceivable. The psychological laboratory experiment, however, shows that the whole psychophysical effect depends upon the character of the signal; a more intense light, a quicker change, a higher tone, a larger field of light, a louder noise, or a harder touch may produce a very different kind of reaction.

With a careful time-measurement of the motions, it can often be directly traced how purely technical processes in the machine itself influence and control the whole psychical system of impulses in the man. I observed, in a factory, for instance, the work at a machine which performed most of its functions automatically. It had to hammer fine grooves into small metal plates. A young laborer stood before every such machine, took from a pile, alternately from the right and from the left, the little plates to be serrated, placed them in the machine, turned a lever to bring the hammer into motion, and then removed

the serrated plates. The speed of the work was dependent upon the operative, as he determined by his lever movement the instant at which the automatic serrating hammer should be released. The man's activity demanded 9 independent movements. I found that those who worked the most quickly were able to carry out this labor for hours at a uniform rapidity of 4 to $4\frac{1}{2}$ seconds for those 9 movements. But the time-measurement showed that even these fastest workers were relatively slow in the first 5 movements which they made while the machine stood quiet, and that they reached an astonishing quickness of movement in the 4 last actions during which at the same time the serrating hammer in bewildering rapidity was beating on the plate with sharp loud cracks. The hammer reinforced the energy of the young laborers to an effectiveness which could never have been attained by mere voluntary effort.

Often the simplicity or complication of the stimulus may be decisive in importance, and this also holds true where the most elementary reactions are involved, for instance, the mere act of counting which enters into many industrial functions. Experiments carried on in my laboratory [31] have shown that the time needed to count a certain number of units becomes longer

as soon as the units themselves become more complicated. Their inner manifoldness exerts a retarding influence on the eye as it moves from one figure to another. A certain psychical inhibition arises; the mind is held back by the complexity of the impression and cannot proceed quickly enough to the next. Psychologically no less important is the demand that the external technical conditions so far as they influence consciousness, should remain as far as possible the same, if the same psychical effect is desired, because then only can a perfectly firm connection between stimulus and movement be formed. In technical life this demand is much sinned against. A typical case is that of the signals for which the engineer on the locomotive has to watch. In the daytime the movable arms of the semaphore indicate by their horizontal, oblique, or vertical position whether the tracks are clear. At nighttime, on the other hand, the same information reaches him by the different colors of the signal lanterns. From a psychical point of view it is probable that the safety of the service would be increased if an unchangeable connection between signal and movement were formed. It would be sufficient for that purpose if the color signals at night were given up and were replaced by horizontal, oblique, or vertical lines of white light or

rows of points. Successful experiments of this kind have been carried on by psychologists in the service of this railroad problem.[32]

The interest in all these problems of large concerns, in transportation and factory work and complex industries, ought not to make us overlook the fact that on principle the same problems can be found in the simplest industrial establishment. Even the housewife or the cook destroys economic values if daily she has to spend useless minutes or hours on account of arrangements in the household which are badly adjusted to the psychological conditions. She sacrifices her energy in vain and she wastes her means where she herself is under the illusion of especial economy. Scientific management would perhaps be nowhere so wholesome as in kitchen and pantry, in laundry and cellar, just because here the saving would be multiplied millionfold and the final sum of energy saved and of feeling values gained would be enormous, even if it could not be calculated with the exactitude with which the savings of a factory budget can be proven. The profusion of small attractive devices which automatically perform the economic household labor and disburden the human workers must not hide the fact that the chief activities are still little adjusted to the psychophysical conditions. The situation

is similar to that of the masons, whose function has also been performed for thousands of years, and yet which did not find a real adaptation to the psychical factors until a systematic time-measuring study was introduced. A manufacturer who sells an improved pan or mixing-spoon or broom expects success if he brings to the market something the merits of which are evident and make the housewife anticipate a decrease of work or a simplification of work, but the development of scientific management has shown clearly that the most important improvements are just those which are deduced from scientific researches, without at first giving satisfaction to the laborers themselves, until a new habit has been formed.

Perhaps the most frequent technical activity of this simple kind is sewing by hand, which is still entirely left to the traditions of common sense, and yet which is evidently dependent upon the interplay of many psychical factors which demand a subtle adaptation to the psychical conditions. To approach, at least, this field of human labor a careful investigation of the psychophysics of sewing has been started in my laboratory.[33] The sewing work is done, with the left hand supported, and the right hand connected with a system of levers which make a graphic record of every movement on the smoked surface of a re-

volving drum. For instance, we begin with simple over and over stitches, measuring the time and the character of the right hand movements for 50 stitches under a variety of technical conditions. The first variation refers to the length of the thread. The thread itself, fixed at the needle's eye, varied between 3 feet and 6 inches in length. Other changes refer to the voluntary speed, to the number of stitches, to fatigue, to external stimuli, to attention, to methods of training, and so on, but the chief interest remains centred on the psychical factors. We are still too much at the beginning already to foresee whether it will be possible to draw from these psychophysical experiments helpful conclusions. The four young women engaged in this laboratory research will later extend it to the psychological conditions of work with the various types of sewing-machines.

XV

THE ECONOMY OF MOVEMENT

THE study of the technical aspect of labor can nowhere be separated by a sharp demarcation line from the study of the labor itself as a function of the individual organism. Many problems, indeed, extend in both directions. The student of industrial efficiency is, for instance, constantly led to the question of fatigue. He may consider this fatigue as a function of brain and muscle activity and discuss it with reference to the psychophysical effort, but he is equally interested in the question of how far the apparatus or the machine or the accessory conditions of the work might be changed in order to avoid fatigue. The accidents of the electric street railways were regarded as partly related to fatigue. The problem was accordingly how to shorten the working time of the motormen in the interest of the public, but it was soon recognized that the difficulty might also be approached from the mere technical side. Some companies introduced seats which the motormen can use whenever they feel fatigue coming and excellent results have followed this innovation. In our last discussions the technical

apparatus stood in the foreground. We may now consider as our real topic the psychophysical activity.

Here, too, the leaders of scientific management have secured some signal successes. Their chief effort in this field was directed toward the greatest possible achievement by eliminating all superfluous movements and by training in those movement combinations which were recognized as the most serviceable ones. We may return to the case of the masons in order to clear up the principle. When Gilbreth began to reform the labor of the mason after scientific principles, he gave his chief interest to the men's motions. Every muscle contraction which was needed to move the brick from the pile in the yard to the final position in the wall was measured with reference to space- and time-relations and the necessary effort. From here he turned to the application of well-known psychophysical principles. A movement is less fatiguing and therefore economically most profitable if it occurs in a direction in which the greatest possible use of gravitation can be made. If both hands have to act at the same time, the labor can be carried out most quickly and with the smallest effort if corresponding muscle groups are at work and this means if symmetrical movements are performed. If unequal movements have to

be made simultaneously, the effort will become smaller if they are psychically bound together by a common unified impulse. The distance which has to be overcome by hands, arms, or feet must be brought to a minimum for each partial movement. Most important, however, is this rule. If a definite combination of movements has been determined as economically most suitable, this method must be applied without any exception from the beginning of the learning. The point is to train from the start those impulse combinations which can slowly lead to the quickest and best work. The usual method is the opposite. Generally the beginner learns to produce from the beginning work which is as good and correct as possible. In order to produce such qualitatively good results at an early stage, it is left to him to choose any groups of movements which happen to be convenient to him. Then these become habitual, and as soon as he tries to go on to quicker work, these chance habits hinder him in his progress. The movements which may be best suited for fair production by a beginner may be entirely unsuited for really quick work, such as would be expected from an experienced man. The laborer must replace the first habits which he has learned by a new set, instead of starting in the first place with motions which can be continued

until the highest point of efficiency has been reached, even if this involves rather a poor showing at the beginning. A final maximum rapidity must be secured from the start by the choice of those motions which have been standardized by careful experiments.

It is also psychophysically important to demand that the movements shall not be suddenly stopped, if that can be avoided. Any interruption of a movement presupposes a special effort of the will which absorbs energy, and after the interruption a new start must be made of which the same is true. On the other hand, if chains of movements become habitual, the psychophysical effort will be reduced to the minimum, inasmuch as each movement finds its natural end and is not artificially interrupted by will, and at the same time each movement itself becomes a stimulus for the next movement by its accompanying sensations. The traditional method, for instance, demands that a brick be lifted with one hand and a trowel with mortar by the other hand. After that the lifting movement is interrupted, the brick comes to rest in the hand of the mason until the mortar has been spread on and the place prepared for the new brick. Then only begins a new action with the brick. This method was fundamentally changed. The laborers learned to swing the brick

with one hand from the pack to the wall and at the same time to distribute the mortar over the next brick with the other hand. This whole complex movement is of course more difficult and demands a somewhat longer period of learning, but as soon as it is learned an extreme saving of psychophysical energy and a correspondingly great economic gain is secured. The newly trained masons are not even allowed to gather up with the trowel any mortar which falls to the floor, because it was found that the loss of mortar is economically less important than the waste of psychophysical energy in bending down.

Whoever has once schooled his eye to observe the limitless waste of human motions and psychophysical efforts in social life has really no difficulty in perceiving all this at every step. This ability to recognize possible savings of impulse may be brought to a certain virtuosity. Gilbreth, one of the leaders of the new movement, seems to be such a virtuoso. When he was in London, there was pointed out to him in the Japanese British Exhibition a young girl who worked so quickly that there at least he would find a rhythm of finger movement which could not any further be improved. In an exhibition booth the woman attached advertisement labels to boxes with phenomenal rapidity. Gilbreth watched her for a little

while and found that she was able to manage 24 boxes in 40 seconds. Then he told the young girl that she was doing it wrongly, and that she ought to try a new way which he showed her. At the first attempt, she disposed of 24 boxes in 26 seconds and at the second trial in 20 seconds. She did not have to make more effort for it, but simply had fewer movements to make. If such economic gain can be secured with little exertion in the simplest processes, it cannot be surprising that in the case of more complex and more advanced technical work which involves highly skilled labor, a careful psychophysical study of motions must bring far-reaching economic improvements.

Yet the more important steps will have to be guided by special experimental investigations, and here the psychological laboratory must undertake the elaboration of the details. Only the systematic experiment can determine what impulses can be released at the least expense of energy and with the greatest exactitude of the motor effect. Investigations on the psychophysics of movement and the influences which lead toward making the movement too large or too small have played an important rôle in the psychological laboratories for several decades. It was recognized early that the mistakes which are made in reproducing a movement may spring from two different sources.

They result partly from an erroneous perception or memory of the movement carried out, and partly from the inability to realize the movement intention. One series of investigations was accordingly devoted to the studies of those sensations and perceptions by which we become aware of the actual movement. Everything which accentuates these sensations must lead to an overestimation of the motion, and the outcome is that the movement is made too small. The concentration of attention, therefore, has the effect of reducing the actual motion, and the same influence must result from any resistance which is not recognized as such and hence is not subtracted in the judgment of the perceiver. Another series of researches was concerned with the inner attitude which causes a certain external movement effect and which may lead to an unintended amount of movement as soon as the weight to be lifted is erroneously judged upon. Closely related studies, finally, deal with a mistake which enters when the movement is reproduced from memory after a certain time. The exactitude of a simple arm movement seems to increase in the first ten seconds, then rapidly to decrease. The emotional attitude, too, is of importance for the reproduction of a movement. I trained myself in making definite extensor and flexor movements of the

arm until I was able to reproduce them under normal conditions with great exactitude. In experiments extending over many months, which were carried on through the changing emotional attitudes of daily life, the exact measurement showed that both groups of movements became too large in states of excitement and too small in states of fatigue. But in a state of satisfaction and joy the extensor movement became too large, the flexor movement too small, and *vice versa,* in unpleasant emotional states the flexor movement was too strong and the extensor movement too weak.[34]

We have a very careful investigation into the relations between rapidity of movement and exactitude.[35] The subjects had to perform a hand movement simultaneously with the beat of a metronome, the beats of which varied between 20 and 200 in the minute. In general the accuracy of the movement decreases as the rapidity increases, but the descent is not uniform. Motions in the rhythm of 40 to the minute were on the whole just as exact as those in the rhythm of 20, and, on the other hand movements in the rhythm of 200 almost as accurate as those of 140 to the minute. Thus we have a lower limit below which decrease of rapidity does not increase the accuracy any further, and an upper limit beyond which a further increase of rapidity brings no addi-

tional deterioration. The mistakes of the unskilled left hand increase still more rapidly than the number of movements. If the eyes are closed, the rapid movements are usually too long and the slow ones too short.

An investigation in the Harvard laboratory varied this problem in a direction which brings it still nearer to technical conditions of industry. Our central question was whether the greatest exactitude of rhythmical movement is secured at the same rapidity for different muscle groups.[36] We studied especially rhythmical movements of hand, foot, arm, and head, and studied them, moreover, under various conditions of resistance. The result from 340,000 measured movements was the demonstration that every muscle group has its own optimum of rapidity for the greatest possible accuracy and that the complexity of the movement and the resistance which it finds has most significant influence on the exactitude of the rhythmical achievement. If we abstract at first from the fluctuations around the average value of a particular group of movements and consider only this average itself in its relation to the starting movement which it is meant to imitate, we find characteristic tendencies toward enlargement or reduction dependent upon the rapidity. The right foot, for instance, remained nearest

to the original movement at a rapidity of 80 motions in the minute, while the head did the same at about 20. For a hand movement of 14 centimeters, the most favorable rapidity was 120 repetitions in the minute, while for a hand movement of 1 centimeter the average remained nearest to the standard at about 40 repetitions. The mean variation from the average is the smallest for the left foot at 20 to 30 movements, for the right at 160 to 180, for the head at 40, for the larger hand movement at 180, and so on. Investigations of this kind have so far not affected industrial life in the least, but it seems hardly doubtful that a systematic study of the movements necessary for economic work will have to pass through such strictly experimental phases. The essential point, however, will be for the managers of the industrial concerns and the psychological laboratory workers really to come nearer to each other from the start and undertake the work in common, not in the sense that the laboratory is to emigrate to the factory, but in the better sense that definite questions which grow out of the industrial life be submitted to the scientific investigation of the psychologists.

XVI

EXPERIMENTS ON THE PROBLEM OF MONOTONY

THE systematic organization of movements with most careful regard to the psychophysical conditions appeared to us the most momentous aid toward the heightening of efficiency. But even if the superfluous, unfit, and interfering movement impulses were eliminated and the conditions of work completely adjusted to the demands of psychology, there would still remain a large number of possibilities through which productiveness might be greatly decreased, or at least kept far below the possible maximum of efficiency. For instance, even the best adapted labor might be repeated to the point of exhaustion, at which the workman and the work would be ruined. Fatigue and restoration accordingly demand especial consideration. In a similar way emotions may be conditions of stimulation or interference, and no one ought to underestimate the importance of higher motives, intellectual, æsthetic, and moral motives, in their bearing on the psychophysical impulses of the laborer. If these higher demands are satisfied, the whole system gains a new tonus, and if they are disappointed, the irri-

tation of the mental machinery may do more harm than any break in the physical machine at which the man is working. In short, we must still look in various directions to become aware of all the relations between the psychological factors and the economic output. We may begin with one question which plays a large, perhaps too large, rôle in the economic and especially in the popular economic literature. I refer to the problem of monotony of labor.

In the discourses of our time on the lights and shades of our modern industrial life, all seem to agree that the monotony of industrial labor ought to be entered on the debit side of the ledger of civilization. Since the days when factories began to spring up, the accusation that through the process of division of labor the industrial workingman no longer has any chance to see a whole product, but that he has to devote himself to the minutest part of a part, has remained one of the matter-of-course arguments. The part of a part which he has to cut or polish or shape in endless repetition without alteration cannot awake any real interest. This complete division of labor has to-day certainly gone far beyond anything which Adam Smith described, and therefore it now appears undeniable that the method must create a mental starvation which presses down the whole

life of the laborer, deprives it of all joy in work, and makes the factory scheme a necessary but from the standpoint of psychology decidedly regrettable evil. I have become more and more convinced that the scientific psychologist is not obliged to endorse this judgment of popular psychology.

To be sure the problem of division of labor, as it appears in the subdivision of manufacture, is intimately connected with many other related questions. It quickly leads to the much larger question of division of labor in our general social structure, which is necessary for our social life with its vocational and professional demands, and which undoubtedly narrows to a certain degree every individual in the completeness of his human desires. No man in modern society can devote himself to everything for which his mind may long. But as a matter of course these large general problems of civilization lie outside of the realm of our present inquiry. In another direction the problem of monotony comes very near to the question of fatigue. But we must see clearly that these two questions are not identical and that we may discuss monotony here without arguing the problem of fatigue. The frequent repetition of the same movement or of the same mental activity certainly may condition an object-

ive fatigue, which may interfere with the economic output, but this is not the real meaning of the problem of monotony. About fatigue we shall speak later. Here we are concerned exclusively with that particular psychological attitude which we know as subjective dislike of uniformity and lack of change in the work. Within these limits the question of monotony is, indeed, frequently misunderstood in its economic significance.

Let us not forget that the outsider can hardly ever judge when work offers or does not offer inner manifoldness. If we do not know and really understand the subject, we are entirely unable to discriminate the subtler inner differences. The shepherd knows every sheep, though the passer-by has the impression that they all look alike. This inability to recognize the differences which the man at work feels distinctly shows itself even in the most complicated activities. The naturalist is inclined to fancy that the study of a philologist must be endlessly monotonous, and the philologist is convinced that it must be utterly tiresome to devote one's self a life long to some minute questions of natural science. Only when one stands in the midst of the work is he aware of its unlimited manifoldness, and feels how every single case is somehow different from every other.

In the situation of the industrial workman, the attention may be directed toward some small differences which can only be recognized after long familiarity with the particular field. Certainly this field is small, as every workman must specialize, but whether he manufactures a whole machine, or only a little wheel, makes no essential difference in the attitude. The attraction of newness is quickly lost also in the case of the most complicated machine. On the other hand, the fact that such a machine has an independent function does not give an independent attraction to the work. Or we might rather say, as far as the work on a whole machine is of independent value, the work of perfecting the little wheel is an independent task also and offers equal value by its own possibilities. Whoever has recognized the finest variations among the single little wheels and has become aware of how they are produced sometimes better, sometimes worse, sometimes more quickly, sometimes more slowly, becomes as much interested in the perfecting of the minute part as another man in the manufacture of the complex machine. It is true that the laborer does not feel interest in the little wheel itself, but in the production of the wheel. Every new movement necessary for it has a perfectly new chance and stands in new relations, which have nothing to do

with the repetition. As a matter of course this interest in the always new best possible method of production is still strongly increased where piece-wages are introduced. The laborer knows that the amount of his earning depends upon the rapidity with which he finishes faultless products. Under this stimulus he is in a continuous race with himself, and thus has every reason to prefer the externally uniform and therefore perfectly familiar work to another kind which may bring alternation, but which also brings ever new demands.

For a long while I have tried to discover in every large factory which I have visited the particular job which from the standpoint of the outsider presents itself as the most tiresome possible. As soon as I found it, I had a full frank talk with the man or woman who performed it and earnestly tried to get self-observational comment. My chief aim was to bring out how far the mere repetition, especially when it is continued through years, is felt as a source of discomfort. I may again point to a few chance illustrations. In an electrical factory with many thousands of employees I gained the impression that the prize for monotonous work belonged to a woman who packs incandescent lamps in tissue paper. She wraps them from morning until night, from the first

day of the year to the last, and has been doing that for the last 12 years. She performs this packing process at an average rate of 13,000 lamps a day. The woman has reached about 50,000,000 times for the next lamp with one hand and with the other to the little pile of tissue sheets and then performed the packing. Each lamp demands about 20 finger movements. As long as I watched her, she was able to pack 25 lamps in 42 seconds, and only a few times did she need as many as 44 seconds. Every 25 lamps filled a box, and the closing of the box required a short time for itself. She evidently took pleasure in expressing herself fully about her occupation. She assured me that she found the work really interesting, and that she constantly felt an inner tension, thinking how many boxes she would be able to fill before the next pause. Above all, she told me that there is continuous variation. Sometimes she grasps the lamp or paper in a different way, sometimes the packing itself does not run smoothly, sometimes she feels fresher, sometimes less in the mood for the work, and there is always something to observe and something to think about.

This was the trend which I usually found. In some large machine works I sought for a long time before I found the type of labor which seemed to me the most monotonous. I finally

settled on a man who was feeding an automatic machine which was cutting holes in metal strips and who simply had to push the strips slowly forward; only when the strip did not reach exactly the right place, he could stop the automatic machine by a lever. He made about 34,000 uniform movements daily and had been doing that for the past 14 years. But he gave me the same account, that the work was interesting and stimulating, while he himself made the impression of an intelligent workingman. At the beginning, he reported, the work had sometimes been quite fatiguing, but later he began to like it more and more. I imagined that this meant that at first he had to do the work with full attention and that the complex movement had slowly become automatic, allowing him to perform it like a reflex movement and to turn his thoughts to other things. But he explained to me in full detail that this was not the case, that he still feels obliged to devote his thoughts entirely to the work at hand, and that he is able only under these conditions to bring in the daily wage which he needs for his family, as he is paid for every thousand holes. But he added especially that it is not only the wage which satisfies him, but that he takes decided pleasure in the activity itself.

On the other hand, I not seldom found wage-

earners, both men and women, who seemed to have really interesting and varied activities and who nevertheless complained bitterly over the monotonous, tiresome factory labor. I became more and more convinced that the feeling of monotony depends much less upon the particular kind of work than upon the special disposition of the individual. It cannot be denied that the same contrast exists in the higher classes of work. We find school-teachers who constantly complain that it is intolerably monotonous to go on teaching immature children the rudiments of knowledge, while other teachers with exactly the same task before them are daily inspired anew by the manifoldness of life in the classroom. We find physicians who complain that one case in their practice is like another, and judges who despair because they always have to deal with the same petty cases, while other judges and physicians feel clearly that every case offers something new and that the repetition as such is neither conspicuous nor disagreeable. We find actors who feel it a torture to play the same rôle every evening for several weeks, and there are actors who, as one of the most famous actresses assured me after the four hundredth performance of her star rôle, repeat their parts many hundred times with undiminished interest, because they feel that

they are always speaking to new audiences. It seems not impossible that this individual difference might be connected with deeper-lying psychophysical conditions. I approached the question, to be sure, with a preconceived theory. I fancied that certain persons had a finer, subtler sense for differences than others and that they would recognize a manifoldness of variations where the others would see only uniformity. In that I silently presupposed that the perception of the uniformity must be something disturbing and disagreeable and the recognition of variations something which stimulates the mind pleasantly. But when I came to examine the question experimentally, I became convinced that such a hypothesis is erroneous, and if I interpret the results correctly, I should say that practically the opposite relation exists. Those who recognize the uniformities readily are not the ones who are disturbed by them.

I proceeded in the following way. To make use of a large number of subjects accustomed to intelligent self-observation, I made the first series of experiments with the regular students in my psychology lecture course in Harvard University. Last winter I had more than four hundred men students in psychology who all took part in that introductory series. The task which I put before

them in a number of variations was this: I used lists of words of which half, or one more or less than half, belonged to one single conceptional group. There were names of flowers, or cities, or poets, or parts of the body, or wild animals, and so on. The remaining words of the list, on the other hand, were without inner connection and without similarity. The similar and the dissimilar words were mixed. The subjects listened to such a list of words and then had to decide without counting from the mere impression whether the similar words were more or equally or less numerous than the dissimilar words. In other experiments the arrangement was that two different lists were read and that in the two lists a larger or smaller number of words were repeated from the first list. Here, too, the subjects had to decide from the mere impression whether the repeated words were in the majority or not. In every experiment the judgment referred to those words which belonged to the same group and which were in this sense uniform, or to the repeated words, and it had to be stated with reference to them whether their number was larger, equal to, or smaller than the different words. If all replies had been correct, the judgment would have been 40 per cent equal, 30 per cent smaller, and 30 per cent larger, as they were arranged in perfect symmetry. As soon

as I had the results from the students, we figured out for every one what number he judged equal, smaller, or greater. Then we divided the equal judgments by 2 and added half of them to the larger and half to the smaller judgments. In this way we were enabled by one figure to characterize the whole tendency of the individual. We found that in the whole student body there was a tendency to underestimate the number of the similar or of the repeated words. The majority of my students had a stronger impression from the varying objects than from those which were in a certain sense equal. Yet this tendency appeared in very different degrees and for about a fourth of the participants the opposite tendency prevailed. They received a stronger impression from the uniform ideas.

I had coupled with these experimental tests a series of questions, and had asked every subject to express with fullest possible self-analysis his practical attitude to monotony in life. Every one had to give an account whether in the small habits of life he liked variety or uniform repetition. He was asked especially as to his preferences for or against uniformity in the daily meals, daily walks, and so on. Furthermore he had to report how far he is inclined to stick to one kind of work or to alternate his work, how far he wel-

comes the idea that vocational work may bring repetition, and so on. And finally I tried to bring the results of these self-observations into relation with the results of those experiments. It was here that the opposite of the hypothesis which I had presupposed suggested itself to me with surprising force. I found that just the ones who perceive the repetition least hate it most, and that those who have a strong perception of the uniform impressions and who overestimate their number are the ones who on the whole welcome repetition in life.

As soon as I had reached this first experimental result, I began to see how it might harmonize with known psychological facts. Some years ago a Hungarian psychologist [37] showed by interesting experiments that if a series of figures is exposed to the eye for a short fraction of a second, equal digits are seen only once, and he came to the conclusion that equal impressions in such a series inhibit each other. In the Harvard laboratory we varied these experiments by eliminating the spatial separation of those numbers. In our experiments the digits did not stand side by side, but followed one another very quickly in the same place.[38] Similar experiments we made with colors and so on. Here, too, we found that quickly succeeding equal or very similar impres-

sions have a tendency to inhibit each other or to fuse with each other. Where such an inhibition occurs, we probably ought to suppose that the perception of the first impression exhausts the psychical disposition for this particular mental experience. The psychophysical apparatus becomes for a moment unable to arouse the same impression once more.

The above described new experiments suggest to me that this inhibition of equal or similar impressions is found unequally developed in different individuals. They possess a different tendency to temporary exhaustion of psychophysical dispositions. There are evidently persons who after they have received an impression are unable immediately to seize the same impression again. Their attention and their whole inner attitude fails. But there are evidently other persons for whom, on the contrary, the experience of an impression is a kind of inner preparation for arousing the same or a similar impression. In their case the psychophysical dispositions become stimulated and excited, and therefore favor the repetition. If, as in our experiments, the task is simply to judge the existence of equal or similar impressions without any strain of attention, the one group of persons must underestimate the number of the equal impressions because many words are

simply inhibited in their minds and remain neglected, the other groups of persons must from their mental dispositions overestimate the number of similar words. From here we have to take one step more. If these two groups of persons have to perform a task in which it is necessary that not a single member of a series of repetitions be overlooked, it is clear that the two groups must react in a very different way. Now a perfect perception of every single member is forced on them. Those who grasp equal impressions easily, and who are prepared beforehand for every new repetition by their inner dispositions, will follow the series without strain and will experience the repetition itself with true satisfaction. On the other hand, those in whom every impression inhibits the readiness to receive a repetition, and whose inner energy for the same experience is exhausted, must feel it as a painful and fatiguing effort if they are obliged to turn their attention to one member after another in a uniform series. This mental torture is evidently the displeasure which such individuals call the dislike of monotony in their work. Whether this theoretical view is correct, we have to determine by future studies. In our Harvard laboratory we have now proceeded from such preparatory mass experiments to subtle investigations on a small number of

persons well trained in psychological self-observation with whom the conditions of the experiment can be varied in many directions.[39]

It would seem probable that such experiments might also win psychotechnical significance. A short series of tests which would have to be adapted to the special situations, and which for the simple wage-earner would have to be much easier than those sketched above, would allow it to be determined beforehand whether an individual will suffer from repetition in work. Even if we abstract from arguments of social reform and consider exclusively the economic significance, it must seem important that labor which involves much repetition be performed by men and women whose mental dispositions favor an easy grasp of successive uniform impressions. Experimentation could secure the selection of the fit workmen and the complaint of monotony would disappear. The same selection could be useful in the opposite direction, as many economic occupations, especially in our time of automatic machines, demand a quick and often rhythmical transition from one activity to another. It is evident that those whose natural dispositions make every mental excitement a preparation not for the identical but for the contrasting stimulation will be naturally equipped for this kind of economic tasks.

XVII

ATTENTION AND FATIGUE

THE problem of monotony may lead us on to other conditions through which attention is hindered and the product of labor thereby decreased. The psychologist naturally first thinks of external distractions of attention. If he turns to practical studies of the actual economic life, he is often decidedly surprised to find how little regard is given to this psychophysical factor. In industrial establishments in which the smallest disturbance in the machine is at once remedied by a mechanic in order that the greatest possible economic effect may be secured, frequently nobody takes any interest in the most destructive disturbances which unnecessarily occur in the subtlest part of the factory mechanism, namely, the attention apparatus of the laborers. Such an interference with attention must, for instance, be recognized when the workingman, instead of devoting himself to one complex function, has to carry out secondary movements which appear to be quite easily performed and not to hinder him in his chief task. Often his own feeling may endorse this impression. Of course the individual

differences in this direction are very great. The faculty of carrying on at the same time various independent functions is unequally distributed and the experiment can show this clearly. It is also well known from practical life that some men can easily go on dictating to a stenographer while they are affixing their signature to several hundred circular letters, or can continue their fluent lecture while they are performing experimental demonstrations. With others such a side activity continually interrupts the chief function. Then some succeed better than others in securing a certain automatism of the accessory function to such a point that its special acts do not come to consciousness at all. For example, I watched a laborer who was constantly engaged in a complicated technical performance, and he seemed to give to it his full attention. Nevertheless he succeeded in moving a lever on an automatic machine which stood near by whenever a certain wheel had made fifty revolutions. During all his work he kept counting the revolutions without being conscious of any idea of number. A system of motor reactions had become organized which remained below the threshold of consciousness and which produced only at the fiftieth recurrence the conscious psychical impulse to perform the lever movement. Yet whether the talent

for such simultaneous mastery of independent functions be greater or smaller and the demand more or less complex, in every case the principal action must be hampered by the side issue. To be sure, it may sometimes be economically more profitable to allow the hindrance to the chief work in order to save the expense of an extra man to do the side work. In most cases, however, such a consideration is not involved; it is simply an ignoring of the psychological situation. As the accessory work seems easy, its hindering influence on other functions is practically overlooked. Psychological laboratory experiments have shown in many different directions that simultaneous independent activities always disturb and inhibit one another.

We must not forget that even the conversations of the laborers belong in this psychophysical class. Where a continuous strain of attention has produced a state of fatigue, a short conversation will bring a certain relief and relaxation, and the words which the speaker hears in reply will produce a general stimulation of psychical energy for the moment. Moreover, the mere existence of the social conversational intercourse will raise the general emotional mood, and this feeling of social pleasure may be the source from which may spring new psychophysical powers. Nevertheless the

fundamental fact, after all, is that any talking during the labor, so far as it is not necessary for the work itself, surely involves a distraction of attention. Here, too, the individual is not conscious of the effect. He feels certain that he can perform his task just as well, and even the pieceworker, who is anxious to earn as much as possible, is convinced that he does not retard himself by conversation. But the experiments which have been carried on in establishments with scientific management speak decidedly against such a supposition. A tyrannical demand for silence would, of course, be felt as cruelty, and no suggestion of a jail-like discipline would be wise in the case of industrial labor, for evident psychological reasons. But various factories in rearranging their establishments according to the principles of scientific management have changed the positions of the workmen so that conversations become more difficult or impossible. The result reported seems to be everywhere a significant increase of production. The individual concentrates his mind on the task with an intensity which seems beyond his reach as long as the inner attitude is adjusted to social contact. The help which is rendered by the feeling of social coöperation, on the other hand, is not removed by the mere abstaining from speaking. Interesting psy-

chopedagogical experiments have, indeed, demonstrated that working in a common room produces better results than isolated activity. This is not true of the most brilliant, somewhat nervous school children, who achieve in their own room at home more than in the classroom. But for the average, which almost alone is in question for life in the factory, the consciousness of common effort is a source of psychophysical reinforcement. This evidently remains effective when the workingmen can see one another, even if the arrangement of the seats precludes the possibility of chatting during the work.

However, by far the more important cause of distraction of attention lies in those disturbances which come from without. Here again the chief interest ought to be attached to those interferences which the workman himself no longer feels as such. In a great printing-shop a woman who was occupied with work which demanded her fullest attention was seated at her task in an aisle where trucking was done. Removing this operator to a quiet corner caused an increase of 25 per cent in her work.[40] To be sure there are many such disturbances in factory life which can hardly be eliminated with the technical means of to-day. For instance, the noise of the machines, which in many factories makes it impossible to

communicate except by shouting, must be classed among the real psychological interferences in spite of the fact that the laborers themselves usually feel convinced that they no longer notice it at all. Still more disturbing are strong rhythmical sounds, such as heavy hammer blows which dominate the continuous noises, as they force on every individual consciousness a psychophysical rhythm of reaction which may stand in strong contrast to that of a man's own work. From the incessant inner struggle of the two rhythms, the one suggested by the labor, the other by the external intrusion, quick exhaustion becomes unavoidable.

If it were our purpose to elaborate a real system of psychological economics, we should have to proceed here to a careful study of the influences of fatigue on the industrial achievement. We should have to discuss the various kinds of fatigue and exhaustion, the conditions of restoration, and the whole group of related problems of psychophysics. But this is the one field which has been thoroughly ploughed over by science and by practical life in the course of the last decades. No new suggestion and no new hint of the importance of the problem is needed here. Our short discussion was planned to be confined to those regions which have not been worked up in

systematic investigations and for which new devices seemed desirable. Hence we do not reproduce here the rich material of facts which the physiologists and psychophysicists have brought together in the last half-century, the importance of which for industrial labor is perfectly evident. Moreover, the practical applications and the insight into the social needs have transformed the factories themselves into one big laboratory in which the problem of fatigue has been studied by practical experiments. The problem of the dependence of fatigue and output upon the length of the working day has been tested in numberless places with the methods of really exact research, as it was easy to find out how the achievement of the laborers became quantitatively and qualitatively changed by the shortening of the working hours.

When in one civilized country after another the exhaustingly long working days of the industrial wage-earner were shortened more and more, the theoretical discussions of the legislators and of the social reformers were soon supplemented by careful statistical inquiries in the factories. It was found that everywhere, even abstracting from all other cultural and social interests, a moderate shortening of the working day did not involve loss, but brought a direct gain. The Ger-

man pioneer in the movement for the shortening of the workingman's day, Ernst Abbé, the head of one of the greatest German factories, wrote many years ago that the shortening from nine to eight hours, that is, a cutting-down of more than 10 per cent, did not involve a reduction of the day's product, but an increase, and that this increase did not result from any supplementary efforts by which the intensity of the work would be reinforced in an unhygienic way.[41] This conviction of Abbé still seems to hold true after millions of experiments over the whole globe. But the problem of fatigue has forced itself on the consideration of the men of affairs from still another side. It has been well known for a long while how intimate the relations are between fatigue and industrial accidents. The statistics of the various countries and of the various industries do not harmonize exactly, but a close connection between the number of accidents and the hours of the day can be recognized everywhere. Usually the greatest number of injuries occurs between ten and eleven o'clock in the forenoon and between three and four o'clock in the afternoon. The different distribution of the working hours, and of the pauses for the meals, make the various statistical tables somewhat incomparable. But it can be traced everywhere that in the first working hours

in which fatigue does not play any considerable rôle, the number of accidents is small, and that this number sinks again after the long pauses. It is true that the number also becomes somewhat smaller at the end of the forenoon and of the afternoon period, but this seems to have its cause in the fact that with growing fatigue and with the feeling that the end of the working period is near, the rhythm of the activity becomes much slower, and with such slower movements the danger of accidents is greatly reduced. In a similar way the factories have had to give the fullest attention to the fatigue problem in its relation to the distribution of pauses, and above all in its relation to the advisable speed of the machines, the limits of which are set by the fatigue of the workingmen, and still more of the working-women.

The legislatures, the labor unions, and the manufacturers have then had this problem of fatigue constantly before their eyes.[42] On the other hand, the psychologists and physiologists have continuously studied the fatigue and restoration of the muscle system and of the central nervous system, and have analyzed the facts with the subtlest methods. Yet, in spite of this, it cannot be denied that a real mutual enrichment has so far hardly been in question. On the contrary, the whole situation has again demonstrated the old

experience. The mere trying and trying again in practical life can never reach the maximum effects which may be secured by systematic, scientifically conducted efforts. On the other side the studies of the theoretical scholars can never yield the highest values for civilization if the problems which offer themselves in practical life are ignored. The theorists have to prepare the ground, and in this preparatory work they must, indeed, remain utterly regardless of any practical situations. But after that a second stage must be reached at which on the foundation of this neutral research special theoretical investigations are undertaken which originate from practical conditions. As long as industrial managers have no contact with the experiments of the laboratory and the experimentalists are shy of any contact with the industrial reality, humanity will pass through social suffering. The hope of mankind will be realized by the mutual fertilization of knowing and doing.

The practical efforts of the factories have, indeed, not yet reached the point at which the greatest possible achievement which can be reached without over-fatigue may be secured. We called the abbreviation of the working day an experimental scheme. The question of reducing the working hours is so simple that no further special

experiments are needed. But when we come to the questions of the pauses at work, the speed of work and similar factors related to fatigue, the situation is by far more complicated, and the often capricious changes in the plant have very little in common with a systematic experiment. Some well-known studies of the efficiency engineers clearly demonstrate the possibility of such systematic efforts. The best-known case is probably Taylor's study of the pig-iron handlers of the Bethlehem Steel Company. He found that the gang of 75 men was loading on the average about $12\frac{1}{2}$ tons per man per day. When he discussed with various managers the question of what output would be the possible maximum, they agreed that under premium work, piecework, or any of the ordinary plans for stimulating the men, an output of 18 to 25 tons would be the extreme possibility. Then he proceeded to a systematic study of the fatigue in its relation to the burden and of the best possible relation between working time and resting time. His first efforts to find formulas were unsuccessful, because he calculated only the actual mechanical energy exerted and found that some men were tired after exerting energy of $\frac{1}{8}$ hp., while others seemed to be able to produce the energy of $\frac{1}{2}$ hp. without greater fatigue. But soon he discovered the mistake in his figures.

He had considered only the actual movements, and had neglected the period in which the laborer was not moving and was not exerting energy, but in which a weight was pulling his arms and demanding a corresponding muscular effort. As soon as this muscular achievement was taken into account, too, he found that for each particular weight a definite relation exists between the time that a man is under a heavy load and the time of rest. For the usual loads of 90 pounds, he found that a first-class laborer must not work more than 43 per cent of the working day and must be entirely without load 57 per cent. If the load becomes lighter, the relation is changed. If the workman is handling a half pig weighing 46 pounds, he can be under load 58 per cent of the day and only has to rest during 42 per cent.[43]

As soon as these figures were experimentally secured, Taylor selected fit men, and did not allow them to lift and to carry the loads as they pleased, but every movement was exactly prescribed by foremen who timed exactly the periods of work and rest. If he had simply promised his men a high premium in case they should carry more than the usual 12 tons a day, they would have burdened themselves as heavily as possible and would have carried the load as quickly as possible, thus completely exhausting themselves after three

or four hours of labor. In spite of such senseless exaggeration of effort in the first hours, the total output for the day would have been relatively small. Now the foremen determined exactly when every individual should lift and move the load and when he should sit quietly. The result was that the men, without greater fatigue, were able to carry 47½ tons a day instead of the 12½ tons. Their wages were increased 60 per cent. Such a trivial illustration demonstrates very clearly the extreme difference between an increase of the economic achievement by scientific, experimental investigation and a mere enforcing of more work by artificially whipping-up the mind with promises of extraordinary wages. Yet even such rules as the scientific management engineers have formed, may be elaborated to more lasting prescriptions as soon as the purely psychological factors are brought more into the foreground and are approached with the careful analysis of the experimental psychologist.

Such a systematic psychological inquiry is the more important for questions of fatigue, as we know that the subjective feeling of displeasure in fatigue is no reliable measure for the objective fatigue, that is, for the real reduction of the ability for work. Daily experience teaches us how easily some people overstep the limits of normal

fatigue, and in extreme cases even come to a nervous breakdown because nature did not protect them by the timely appearance of strong fatigue feelings. On the other hand, we find many men and still more women who feel tired even after a small exertion, because they did not learn early to inhibit the superficial feelings of fatigue, or because the sensations of fatigue have in fact a certain abnormal intensity in their case. The question how far the psychophysical apparatus has really been exhausted by a certain effort must be answered with the help of objective research and not on the basis of mere subjective feelings. But such objective measurements demand systematic experiments in the laboratory.

The experiments which really have been carried on in the laboratory as yet, as far as they were not merely physiological, have on the whole been confined to so-called mental labor, and were essentially devoted to problems of school instruction or medical diagnosis. We have no doubt excellent experiments which are devoted to the study of the individual differences of exhaustion, fatigue, exhaustibility, ability to recover the lost energy, ability to learn from practice, and so on, but they are still exclusively adjusted to the needs of the school-teacher and of the nerve specialist and would hardly be immediately useful to the

manager of a factory. We shall need a long careful series of investigations in order to determine how far those manifold results from experiments with memory work, thought work, writing work, and so on can be applied to the work which the industrial laborer is expected to perform.

XVIII

PHYSICAL AND SOCIAL INFLUENCES ON THE WORKING POWER

THE increase and decrease of the ability to do good work depends of course not only upon the direct fatigue from labor and the pauses for rest; a large variety of other factors may lead to fluctuations which are economically important. The various hours of the day, the seasons of the year, the atmospheric conditions of weather and climate, may have such influence. Some elements of this interplay have been cleared up in recent years. Just as the experiments of pedagogical psychology have determined the exact curve of efficiency during the period of an hour in school, so other investigations have traced the typical curve of psychical efficiency throughout the day and the year. Sociological and criminological statistics concerning the fluctuations in the behavior of the masses, common-sense experience of practical life, and finally, economic statistics concerning the quantity and quality of industrial output in various parts of the day and of the year, have supplemented one another. The systematic

assistance of the psychological laboratory, however, has been confined to the educational aspect of the problem. Psychological experiments have determined how the achievement of the youth in the schoolroom changes with the months of the year and the hours of the day. It seems as if it could not be difficult to secure here, too, a connection between exact experiment and economic work. Much will have to be reduced to individual variations. The laboratory has already confirmed the experience of daily life that there are morning workers whose strongest psychophysical efficiency comes immediately after the night's rest, while the day's work fatigues them more and more; and that there are evening workers who in the morning still remain under the after effects of the night's sleep, and who slowly become fresher and fresher from the stimuli of the day. It would seem not impossible to undertake a systematic selection of various individuals under this point of view, as different industrial tasks demand a different distribution of efficiency between morning and night.

Such a selection and adjustment may be economically still more important with reference to the fluctuations during the course of the year. Economic inquiries, for instance, have suggested that younger and older workingmen who ordin-

arily show the same efficiency become unequal in their ability to do good work in the spring months, and the economists have connected this inequality with sexual conditions. But other factors as well, especially the blood circulation of the organism and the resulting reactions to external temperature, different gland activities, and so on, cause great personal differences in efficiency during the various seasons of the year. Inasmuch as we know many economic occupations in which the chief demand is made in one or another period of the year, a systematic study of these individual variations might be of high economic value, where large numbers are involved, and might contribute much to the individual comfort of the workers. But a constant relation to day and year also seems to exist independent of all personal variations. When the sun stands at its meridian, a minimum of efficiency is to be expected and a similar minimum is to be found at the height of summer. Correspondingly we have an increase of the total psychical efficiency in winter-time. During the spring-time the behavior seems, as far as the investigations go, to be different in the intellectual and in the psychomotor activities. It is claimed that the efficiency of the intellectual functions decreases as the winter recedes, but that the efficiency of psychomotor impulses increases.[44]

The influences of the daily temperature, of the weather and of the seasons may be classed among the physical conditions of efficiency. We may group with them the effects of nourishment, of stimulants, of sleep, and so on. As far as the relations between these external factors and purely bodily muscle work are involved, the interests of the psychologists are not engaged. But it is evident that every one of these relations also has its psychological aspect, and that a really scientific psychotechnical treatment of these problems can become possible only through the agency of psychological experiments. We have excellent experimental investigations concerning the influence of the loss of sleep on intellectual labor and on simple psychomotor activities. But it would be rather arbitrary to deduce from the results of those researches anything as to the effect of reduction of sleep on special economic occupations. Yet such knowledge would be of high importance. We have in the literature concerned with accidents in transportation numerous popular discussions about the destructive influence of loss of sleep on the attention of the locomotive engineer or of the helmsman or of the chauffeur, but an analysis of the particular psychophysical processes does not as yet exist and can be expected only from systematic experiments. Nor has the influence

of hunger on psychotechnical activities been studied in a satisfactory way.

A number of psychological investigations have been devoted to the study of the influence of alcohol on various psychical functions and in this field at least the strictly economic problem of industrial labor has sometimes been touched. We have the much quoted and much misinterpreted experiments[45] which were carried on in Germany with typesetters. The workmen received definite quantities of heavy wine at a particular point in the work and the number of letters which they were able to set during the following quarter-hours were measured and compared with their normal achievement in fifteen minutes. The reduction of efficiency amounted on the average to 15 per cent of the output. It may be mentioned that the loss referred only to the quantity of the work and not at all to the quality. The well-known subjective illusory feeling of the subjects was not lacking; they themselves believed that the wine had reinforced their working power. As soon as such experiments are put into the service of economic life, they will have to be carried on with much more accurate adjustment to the special conditions, with subtler gradation of the stimuli, and especially with careful study of individual factors. But at first it seems more in the

interest of the practical task that the extremely complicated problem of the influence of alcohol be followed up by purely theoretical research in the laboratory in order that the effect may be resolved into its various components. We must first find the exact facts concerning the influence of alcohol on elementary processes of mental life, such as perception, attention, memory, and so on, and this will slowly prepare the way for the complete economic experiment.

At present the greatest significance for the economic field may be attached to those alcohol experiments which dealt with the apprehension of the outer world. They proved a reduction in the ability to grasp the impressions and a narrowing of the span of consciousness. The indubitable decrease of certain memory powers, of the acuity in measuring distance, of the time estimation, and similar psychical disturbances after alcohol, must evidently be of high importance for industry and transportation, while the well-known increase of the purely sensory sensibility, especially of the visual acuity after small doses of alcohol, hardly plays an important rôle in practical life. The best-known and experimentally most studied effect of alcohol, the increase of motor excitability, also evidently has its importance for industrial achievements. It cannot be denied that this facil-

ity of the motor impulses after small doses of alcohol is not a real gain, which might be utilized economically, but is ultimately an injury to the apparatus, even if we abstract from the retardation of the reaction which comes as an after-effect. The alcoholic facilitation, after all, reduces the certainty and the perfection of the reaction and creates conditions under which wrong, and this in economic life means often dangerous, motor responses arise. The energy of the motor discharge suffers throughout from the alcohol.

Some experiments which were recently carried on with reference to the influence of alcohol on the power of will seem to have especial significance for the field of economic activity. The method applied in the experiment was the artificial creation of an exactly measurable resistance to the will-impulse directed toward a purpose. The experiment had to determine what power of resistance could be overcome by the will and how far this energy changes under the influence of alcohol. For this end combinations of meaningless syllables were learned and repeated until they formed a close connection in memory. If one syllable was given, the mechanical tendency of the mind was to reproduce the next syllable in the memorized series. The will-intention was then directed toward breaking this memory type. For

instance, it was demanded, when a syllable was called, that the subject should not answer with the next following syllable, but with a rhyming syllable. This will-impulse easily succeeded when the syllables to be learned had been repeated only a few times, while after a very frequent repetition the memory connection offered a resistance which the simple will-intention could not break. The syllable which followed in the series rushed to the mind before the intention to seek a rhyming syllable could be realized. The number of repetitions thus became a measure for the power of the will. After carrying out these experiments at first under normal conditions, they were repeated while the subjects were under the influence of exactly graded doses of alcohol.[46] From such simple tasks the experiment was turned to more and more complex ones of similar structure. All together they showed clearly that the alcohol did not influence the ability to make the will effective and that the actual decrease of achievement results from a decrease in the ability to grasp the material. As long as the alcohol doses are small, this feeling of decreased ability stirs up a reinforcement in the tension of the will-impulse. This may go to such an extent that the increased will-effort not only compensates for the reduced understanding, but even over-compensates for it, producing an im-

provement in the mental work. But as soon as the alcohol doses amount to about 100 cubic centimeters, the increased tension of the will is no longer sufficient to balance the paralyzing effect in the understanding. Yet it must not be overlooked that in all these experiments only isolated will acts were in question which were separated from one another by pauses of rest. Evidently, however, the technical laborer is more often in a situation in which not isolated impulses, but a continuous tension of the will is demanded. How far such an uninterrupted will-function is affected by alcohol has not as yet been studied with the exact means of the experiment.

To be sure an obvious suggestion would be that the whole problem, as far as economics, and especially industry, are concerned, might be solved in a simpler way than by the performance of special psychological experiments, namely, by the complete elimination of alcohol itself from the life of the wage-earner. The laboratory experiment which seems to demonstrate a reduction of objective achievement in the case of every important mental function merely supplements in exact language the appalling results indicated by criminal statistics, disease statistics, and inheritance statistics. It seems as if the time had come when scientists could not with a good

conscience suggest any other remedy than the merciless suppression of alcohol. Indeed, there can be no doubt that alcohol is one of the worst enemies of civilized life, and it is therefore almost with regret that the scientist must acknowledge that all the psychological investigations, which have so often been misused in the partisan writings of prohibitionists, are not a sufficient basis to justify the demand for complete abstinence.

First, newer experiments make it very clear that many of the so-called effects of alcohol which the experiment has demonstrated are produced or at least heightened by influences of suggestion. Experiments which have been carried on in England for the study of that point show clearly that certain psychical disturbances which seem to result from small doses of alcohol fail to appear as soon as the subject does not know that he has taken alcohol. For that purpose it was necessary to eliminate the odor, and this was accomplished by introducing the beverages into the organism by a stomach pump. When by this method sometimes water and sometimes diluted alcohol was given without the knowledge of the subject, the usual effects of small doses of alcohol did not arise. But another point is far more important. We may take it for granted that alcohol reduces the ability for achievement as soon as such very small

doses are exceeded. But from the standpoint of economic life we have no right to consider a reduction of the psychical ability to produce work as identical with a decrease in the economic value of the personality. Such a view would be right if the influence necessarily set in at the beginning of the working period. But if, for instance, a moderate quantity of beer is introduced into the organism after the closing of the working day, it would certainly produce an artificial reduction of the psychical ability, and yet this decrease of psychophysical activity might be advantageous to the total economic achievement of the workingman in the course of the week or the year. To be sure the glass of beer in the evening paralyzes certain inhibitory centres of the brain and therefore puts the mind out of gear, but such a way of expressing it may easily be misleading, as it suggests too much that a real injury is done. From the point of view of scientific psychology, we must acknowledge that such a paralyzing effect in certain parts of the psychophysical system sets in with every act of attention and reaches its climax in sleep, which surely does no harm to the mind. It may be thoroughly advantageous for the total work of the normal, healthy, average workingman if the after effects of the motor excitement of the day are eliminated by a mild, short alcoholic poi-

soning in the evening. It may produce that narrowing and dulling of consciousness which extinguishes the cares and sorrows of the day and secures the night's sleep, and through it increased efficiency the next morning. Systematic experiments with exact relation to the various technical demands must slowly bring real insight into this complex situation. The usual hasty generalization from a few experiments with alcohol for partisan interests is surely not justified in the present unsatisfactory state of knowledge.[47]

Perhaps we know still less of the influences which coffee, tea, tobacco, sweets, and so on exert on the life of the industrial worker. It will be wise to resolve these stimuli in daily use into their elements and to study the effects of each element in isolated form. To know, for instance, the effects of caffein on the psychophysical activities does not mean to know the effects of tea or coffee, which contain a variety of other substances besides the caffein, substances which may be supposed to modify the effect of the caffein. Yet the first step must in this case be the study of the effects of the isolated caffein, before the total influences of the familiar beverages can be followed up. An excellent investigation of this caffein effect on various psychological and psychomotor functions has recently been completed.[48] When

the caffein effect on tapping movements was studied, it was found that it works as a stimulation, sometimes preceded by a slight initial retardation. It persists from one to two hours after doses of from one to three grains and as long as four hours after doses of six grains. The steadiness test showed a slight nervousness after several hours after doses of from one to four grains. After six grains there is pronounced unsteadiness. A complex test in coördination indicated that the effect of small amounts of caffein is a stimulation and that of large amounts a retardation. Correspondingly the speed of performance in typewriting is heightened by small doses of caffein and retarded by larger doses. In both cases the quality of the performance as measured by the number of errors is superior to the normal result.

The influences of the physiological stimulants have many points of contact with the effects of social entertainment, the significance of which for the economic life is still rather unknown in any exact detail. Many factories in which the labor is noiseless, as in the making of cigars, have introduced gramophone music or reading aloud, and it is easy to understand theoretically that a certain animating effect results, which stimulates the whole psychophysical activity. But only the experiment would be able to decide how this stimu-

lation is related, for instance, to the distraction of attention, which is necessarily involved, or how it influences various periods of the work and various types of work, how far it is true that the musical key exerts an exciting or relaxing influence, what intensity and what local position, what rhythm and what duration of such æsthetic stimuli, would bring the best possible economic results. We all have read of the favorable effects which were secured in a factory when a cat was brought into every working-room in which women laborers were engaged in especially fatiguing work. The cat became a living toy for the employees, which stimulated their social consciousness. In not a few plants the reinforced achievement is explained by the social means of entertainment which have been introduced under the pressure of modern philanthropic ideas. The lounging-rooms with the newspapers and periodicals, the clubrooms with libraries, the excursions and dances and patriotic festivities, fill up the reservoir of psychophysical energies. As a matter of course all the social movements which enhance the consciousness of solidarity among the laborers and the feeling of security as to their future development in their career have a similar effect of reinforcing the normal psychical achievement.

As the strongest factor, finally, the direct material interest must be added to these conditions. The literature of political economy is full of discussions of the effect of increase of wages, of the payment of bonuses and premiums, of piece-wages, of promised pensions, and, as far as Europe is concerned, of state insurance. In short, the whole individual financial situation in its relation to the psychophysical achievement of the wage-earner is a favorite topic of economic inquiry. We cannot participate here in these inexhaustible discussions, because all these questions are to-day still so endlessly far from the field of psychological experiments. Nevertheless we ought not to forget the experience through which general experimental psychology has gone in the last few decades. When the first experiments were undertaken in order to deal systematically with the mental life, the friends of this new science and its opponents agreed, on the whole, in the belief that certainly only the most elementary phenomena of consciousness, the sensations and the reactions of impulses, would be accessible to the new method. The opponents naturally compared this modest field with the great problems of the mental totality, and therefore ridiculed the new narrow task as unimportant. The friends, on the other hand, were eager to follow the

fresh path, because they were content to gain real exactitude by the experiment at least in these simplest questions. Yet as soon as the new independent workshops were established for the young science, it was discovered that the method was able to open fields in which no one had anticipated its usefulness. The experiments turned to the problems of attention, of memory, of imagination, of feeling, of judgment, of character, of æsthetic experience and so on. It is not improbable that the method of the economic psychological experiment may also quickly lead beyond the more elementary problems, as soon as it is systematically applied, and then it, too, may conquer regions of inquiry in which to-day no exact calculation of the psychological factors seems possible.

If such an advance is to be a steady one, the economic psychologist will emancipate himself from the chance question of what problems are at this moment important for commerce and industry and will proceed systematically step by step from those results which the psychological laboratory has yielded under the non-economic points of view. Many previous psychological or psychophysical inquiries almost touch the problems of industrial achievement. For instance, the experiments on imitation, which psychophysicists have carried on in purely theoretical

or pedagogical interests, move parallel to industrial experiences. It is well known that the pacemaker plays his rôle not only in the field of sport but also in the factory. The rhythm of one laborer gains controlling importance for the others, who instinctively imitate him. Some plants even have automatically working machines with the special intention that the sharp rhythm of these lifeless forerunners shall produce an involuntary imitation in the psychophysical system. In a similar way many laboratory investigations on suggestion and suggestibility point to such economic processes, and it seems to me that especially the studies on the influence of the ideas of purpose which are being undertaken nowadays in many psychological laboratories may easily be connected with the problems of economic life. We know how the consciousness of the task to be performed has an organizing influence on the system of those psychophysical acts which lead to the goal. The experiment has shown under which conditions this effect can be reinforced and under which reduced. Pedagogical experiments have also shown exactly what influence belongs to the consciousness of the approach to the end of work; the feeling of the nearness of the close heightens the achievement, even of the fatigued subject. It would not be

difficult to connect psychophysical experiments of this kind with the problems of the task and bonus system, which is nowadays so much discussed in industrial life. The practical successes seem to prove that the individual can do more with equal effort if he does not stand before an unlimited mass of work of which he has to do as much as possible in the course of the day, but if he is before a definitely determined, limited task with the demand that he complete it in an exactly calculated time. Scientific management has made far-reaching use of this principle, but whether constant results for the various industries can be hoped for from such methods must again be ascertained by the psychological experiment.

These hopes surely will not weaken the interest of the psychologist for those many psychological methods which lie outside of the experimental research. A sociologist, who himself had been a laborer in his earlier life, undertook in Germany last year an inquiry into the psychological status of the laborers' achievement by the questionnaire method.[49] He sent to 8000 workingmen in the mining industries, textile industries, and metal industries, blanks containing 26 questions, and received more than 5000 replies. The questions referred to the pleasure and interest in the work, to preferences, to fatigue, to the thoughts during

the work, to the means of recreation, to the attitude toward the wages, to the emotional situation, and so on. The 5000 answers allowed manifold classifications. The various mental types of men could be examined, the influence of the machine, the attitude toward monotony, the changes of pleasure and interest in the work with the age of the laborer, the time at which fatigue becomes noticeable, and so on. Many psychological elements of industrial life thus come to a sharp focus and the strong individual differences could not be brought out in a more characteristic way. Yet, all taken together, even such a careful investigation on a psychostatistical basis strongly suggests that a few careful experimental investigations could lead further than such a heaping-up of material gathered from men who are untrained in self-observation and in accurate reports, and above all who are accessible to any kind of suggestion and preconceived idea. The experimental method is certainly not the only one which can contribute to reforms in industrial life and the reinforcement of industrial efficiency, but all signs indicate that the future will find it the most productive and most reliable.

PART III

THE BEST POSSIBLE EFFECT

PART III

THE BEST POSSIBLE EFFECT

XIX

THE SATISFACTION OF ECONOMIC DEMANDS

EVERY economic function comes in contact with the mental life of man, first from the fact that the work is produced by the psyche of personalities. This gave us the material for the first two parts of our discussion. We asked what mind is best fitted for the particular kind of work, and how the mind can be led to the best output of work. But it is evident that the real meaning of the economic process expresses itself in an entirely different contact between work and mind. The economic activity is separated from all other processes in the world, not by the fact that it involves labor and achievement by personalities, but by the fact that this labor satisfies a certain group of human desires which we acknowledge as economic. The mere performance of labor, with all the psychical traits of attention and fatigue and will-impulses and personal qualities, does not in itself constitute anything of economic value.

For instance, the sportsman who climbs a glacier also performs such a fatiguing activity which demands the greatest effort of attention and will; and yet the psychotechnics of sport do not belong in economic psychology, because this mountain climbing does not satisfy economic desires. The ultimate characteristic which designates an activity as economic is accordingly a certain effect on human souls. The whole whirl of the economic world is ultimately controlled by the purpose of satisfying certain psychical desires. Hence this psychical effect is still more fundamental for the economic process than its psychical origin in the mental conditions of the worker. The task of psychotechnics is accordingly to determine by exact psychological experiments how this mental effect, the satisfaction of economic desires, can be secured in the quickest, in the easiest, in the safest, in the most enduring, and in the most satisfactory way.

But we must not deceive ourselves as to the humiliating truth that so far not the slightest effort has been made toward the answering of this central scientific question. If the inquiry into the psychical effects were really to be confined to this problem of the ultimate satisfaction of economic desires, scientific psychology could not contribute any results and could not offer anything but hopes

and wishes for the future. At the first glance it might appear as if just here a large amount of literature exists; moreover, a literature rich in excellent investigations and ample empirical material. On the one side the political economists, with their theories of economic value and their investigations concerning the conditions of prices and the development of luxury, the calculation of economic values from pleasure and displeasure and many similar studies, have connected the economic processes with mental life; on the other side the philosophers, with their theories of value, have not confined themselves to the ethical and æsthetic motives, but have gone deeply into the economic life too. While such studies of the economists and of the philosophers are chiefly meant to serve theoretical understanding, it might seem easy to deduce from them technical practical prescriptions as well. If we know that under particular conditions certain demands will be satisfied, we draw the conclusion that we must realize those conditions whenever such demands are to be satisfied. The theoretical views of the economists and of the philosophers of value might thus be directly translated into psychotechnical advice.

As soon as we look deeper into the situation, we must recognize that this surface impression

is entirely misleading. Certainly whenever the philosophers or political economists discuss the problems of value and of the satisfaction of human demands, they are using psychological terms, but the whole meaning which they attach to these terms, feeling, emotion, will, desire, pleasure, displeasure, joy, and pain, is essentially different from that which controls the causal explanations of scientific psychology. We cannot enter into the real fundamental questions here, which are too often carelessly ignored even in scientific quarters. Too often psychology is treated, even by psychologists, as if it covered every possible systematic treatment of inner experience, and correspondingly outsiders like the economists fancy that they are on psychological ground and are handling psychological conceptions as soon as they make any statements concerning the inner life. But if we examine the real purposes and presuppositions of the various sciences, we must recognize that the human experience can be looked on from two entirely different points of view. Only from one of the two does it present itself as psychological material and as a fit object for psychological study. From the other point of view, which is no less valuable and no less important for the understanding of our inner life, human experience offers itself as a reality with which psychology

as such has nothing to do, even though it may be difficult to eliminate the usual psychological words.

The psychologist considers human experience as a series of objects for consciousness. All the perceptions and memory ideas and imaginative ideas and feelings and emotions, are taken by him as mental objects of which consciousness becomes aware, and his task is to describe and to explain them and to find the laws for their succession. He studies them as a naturalist studies the chemical elements or the stars. It makes no difference whether his explanation leads him to connect these mental contents with brain processes as one theory proposes, or with subconscious processes as another theory suggests. The entirely different aspect of inner life is the one which is most natural in our ordinary intercourse. Whenever we give an account of our inner life or are interested in the experience of our friends, we do not consider how their mental experiences as such objective contents of consciousness are to be described and explained, but we take them as inner actions and attitudes toward the world, and our aim is not to describe and to explain them but to interpret and to understand them. We do not seek their elements but their meaning, we do not seek their causes and effects but their inner rela-

tions and their inner purposes. In short, we do not take them at all as objects but as functions of the subject, and our dealing with them has no similarity to the method of the naturalist.

This method of practical life in which we seek to express and to understand a meaning, and relate every will-act to its aim, is not confined to the mere popular aspect; it can lead to very systematic scholarly treatment. It is exactly the treatment which is fundamental in the case of all history, for example, or of law, or of logic. That is, the historian makes us understand the meaning of a personality of the past and is really interested in past events only as far as human needs are to be interpreted. It would be pseudo-psychology, if we called such an account in the truly historical spirit a psychological description and explanation. The student of law interprets the meaning of the will of the legislator; he does not deal with the idea of the law as a psychological content. And the logician has nothing to do with the idea as a conscious object in the mind; he asks as to the inner relations of it and as to the conclusions from the premises. In short, wherever historical interpretations or logical deductions are needed, we move on in the sphere of human life as it presents itself from the standpoint of immediate true experience without artificially moulding it into the

conceptions of psychology. On the other hand, as soon as the psychological method is applied, this immediate life meaning of human experience is abandoned, and instead of it is gained the possibility of considering the whole experience as a system of causes and effects. Mental life is then no longer what it is to us in our daily intercourse, because it is reconstructed for the purposes of this special treatment, just as the water which we drink is no longer our beverage if we consider it under the point of view of chemistry as a combination of hydrogen and oxygen. Hence we have not two statements one of which is true and the other ultimately untrue; on the contrary, both are true. We have a perfect right to give the value of truth to our experience with water as a refreshing drink, and also to the formula of the chemist. With a still better right we may claim that both kinds of mental experience are equally true. Hence not a word of objection is raised against the discussions of the historians and the philosophers, if we insist that their so-called psychology stands outside of the really descriptive and explanatory account of mental life, and is therefore not psychology in the technical sense of the word.

It is this historical attitude which controls all the studies of the political economists. They

speak of the will-acts of the individuals and of their demands and desires and satisfactions, but they do not describe and explain them; they want to interpret and understand them. They may analyze the motives of the laborer or of the manufacturer, but those motives and impulses interest them not as contents of consciousness, but only as acts which are directed toward a goal. The aim toward which these point by their meaning, and not the elements from which they are made up or their causes and effects, is the substance of such economic studies. For such a subjective account of the meaning of actions the only problem is, indeed, the correct understanding and interpretation, and the consistent psychologist who knows that it is not his task to interpret but to explain has no right to raise any questions here. It is, therefore, only a confusing disturbance, if a really psychological, causal explanation is mixed into the interpretation of such a system of will-acts and purposes. It is true we find this confusion in many modern works on economics. Economists know that a scientific explanatory study of the human mind exists, and they have a vague feeling that they have no right to ignore this real psychology, instead of recognizing that the psychology really has nothing to do with their particular problem. The result is that they constantly

try to discuss the impulses and instincts, the hunger and thirst and sexual desire, and the higher demands for fighting and playing and acquiring, for seeking power and social influence, as a psychologist would discuss them, referring them to biological and physiological conditions and explaining them causally. Yet as soon as they come to their real problems and enter into the interpretation and meaning of these economic energies, they naturally slide back into the historical, economic point of view and discuss the economic relations of men without any reference to their psychologizing preambles. The application of the psychological, scientific method to the true economic experience is therefore not secured at all in this way. The demands and volitions which they disentangle are not the ones which the psychophysiologist studies, because they are left in their immediate form of life reality. They are accordingly inaccessible to the point of view of experimental psychology, and nothing can be expected from such interpretative discussions of the economists for the psychotechnics at which the psychologist is aiming. Even where the political economists deal with the problems of value in exact language, nothing is gained for the kind of insight for which the psychologist hopes, and the psychologists must therefore go on with their

own methods, if they are ever to reach a causal understanding of the means by which a satisfaction of the economic demands may be effected.

So far the psychologists have not even started to examine these economic feelings, demands, and satisfactions with the means of laboratory psychology. Hence no one can say beforehand how it ought to be done and how to gain access to the important problems, inasmuch as the right formulation of the problem and the selection of the right method would here as everywhere be more than half of the solution. It must be left to the development of science for the right starting-point and the right methods to be discovered. Sometimes, to be sure, the experiment has at least approached this group of economic questions. For instance, the investigations of the so-called psychophysical law have often been brought into contact with the experiences of ownership and acquirement. The law, well known to every student of psychology, is that the differences of intensity in two pairs of sensations are felt as equal, when the two pairs of stimuli are standing in the same relation. The difference between the intensities of the light sensations from 10 candles and 11 candles is equal to that from 50 candles and 55 candles, from 100 candles and 110, from 500 candles and 550: that is, the difference of one additional can-

dle between 10 and 11 appears just as great as the difference of 50 candles between 500 and 550. The psychologists have claimed that in a corresponding way the same feeling of difference arises when the amounts of possessions stand in the same relation. That is, the man who owns $100 feels the gain or loss of $1 as much as one who owns $100,000 feels the gain or loss of $1000. Not the absolute amount of the difference, but the relative value of the increase or decrease is the decisive influence on the psychological effect. Some experimental investigations concerning feelings have also come near to the economic boundaries. The study of the contrast feelings and of the relativity of feelings, for instance, has points of contact with the economic problem of how far economic progress, with its stirring up and satisfying of continually new demands, really adds to the quantity of human enjoyment. In other words, how far are those sociologists right who are convinced that by the technical complexity of modern life, with all its comforts and mechanizations, the level of individual life is raised, but that the oscillations about this average level remain the same and produce the same amount of pleasure and pain? The technical advance would therefore bring no increase of human pleasure.

We might also put into this class the meagre

experimental investigations concerning the mutual influence of feelings. When sound, light, and touch impressions, each of which, isolated, produces a feeling of a certain degree, are combined with one another, the experiment can show very characteristic changes in the intensity of pleasure and displeasure. From such routine experiments of the laboratory it might not be difficult to come to more complex experiments on the mutual relations of feeling values and especially of the combinations of pleasure with displeasure. This would lead to an insight into the processes which are involved in the fixing of prices, as they are always dependent upon the pleasure in the acquisition and the displeasure in the outlay. The exact psychology of the future may thus very well determine the conditions under which the best effects for the satisfaction of economic demands may be secured, but our present-day science is still far from such an achievement: and it seems hardly justifiable to propose methods to-day, as it would be like drawing a map with detailed paths for a primeval forest which is still inaccessible.

XX

EXPERIMENTS ON THE EFFECTS OF ADVERTISEMENTS

WE have said that the time has not yet come for discussing from the standpoint of experimental psychology the means to secure the ultimate effects of economic life, namely, the satisfaction of economic demands. If this were the only effect which had economic significance, this whole last part of our little book would have to remain a blank, as we wanted to deal here with the securing of the best effects after having studied the securing of the best man and of the best work. Yet these ultimate ends are certainly not the only mental effects which become important in the course of economic processes. In order to reach that final end of the economic movement, often an unlimited number of part processes distributed over space and time must coöperate. The satisfaction of our thirst in a tea-room may be a trivial illustration of such a final effect, but it is clear that in order to produce this ultimate mental effect of satisfying the thirst, thousands of economic processes must have preceded. To bring the tea and the sugar and the lemon to the table,

the porcelain cup and the silver spoon, wage-earners, manufacturers and laborers, exporters, importers, storekeepers, salesmen, and customers had to coöperate. Among such part processes which serve the economic achievement are always many which succeed only if they produce characteristic effects in human minds. The propaganda which the storekeeper makes, for instance, his display and his posters, serve the economic interplay by psychical effects without themselves satisfying any ultimate economic demand. They must attract the passer-by or impress the reader or stimulate his impulse to buy, and through all this they reach an end which is in itself not final, as no human desire to read advertisements exists. When the salesman influences the customer to buy something which may later help to satisfy a real economic demand, the art of his suggestive words secures a mental effect which again is in itself not ultimate. If the manufacturer influences his employees to work with more attention or with greater industry, or if the community stirs up the desire for luxury or the tendency to saving, we have mental effects which are of economic importance without being really ultimate economic effects.

As far as these effects are necessary and justified stages leading to the ultimate satisfaction of

economic demands, it certainly is the duty of applied psychology to bring psychological experience and exact methods into their service. We emphasize the necessary and justified character of these steps, as it is evident that psychological methods may be made use of also by those who aim toward mental effects which are unjustified and which are not necessary for the real satisfaction of valuable demands. Psychological laws can also be helpful in fraudulent undertakings or in advertisements for unfair competition. The psychotechnical scientist cannot be blamed if the results of his experiments are misused for immoral purposes, just as the chemist is not responsible if chemical knowledge is aplied to the construction of anarchistic bombs. But while psychology, as we have emphasized before, cannot from its own point of view determine the value of the end, the psychologist as a human being is certainly willing to coöperate only where the soundness and correctness of the ends are evident from the point of view of social welfare.

In order to demonstrate the principle of this kind of psychotechnical help with fuller detail, at least by one illustration, I may discuss the case of the advertisements, the more as this problem has already been taken up in a somewhat systematic way by the psychological laboratories. We

have a number of careful experimental investigations referring to the memory-value, the attention-value, the suggestion-value, and other mental effects of the printed business advertisements. Of course this group of experimental investigations at once suggests an objection which we cannot ignore. A business advertisement, as it appears in the newspapers, is such an extremely trivial thing and so completely devoted to the egotistical desire for profit that it seems undignified for the scientist to spend his time on such nothings and to shoot sparrows with his laboratory cannon-balls. But on the one side nothing can be unworthy of thorough study from a strictly theoretical point of view. The dirtiest chemical substance may become of greatest importance for chemistry, and the ugliest insect for zoölogy. On the other side, if the practical point of view of the applied sciences is taken, the importance of the inquiry may stand in direct relation to the intensity of the human demand which is to be satisfied by the new knowledge. Present-day society is so organized that the economic advertisement surely serves a need, and its intensity is expressed by the well-known fact that in every year billions are paid for advertising. Measured by the amount of expenditure, advertising has become one of the largest and economically most important human

industries. It is, then, not astonishing that scientists consider it worth while to examine the exact foundations of this industry, but it is surprising that this industry could reach such an enormous development without being guided by the spirit of scientific exactitude which appears a matter of course in every other large business. As it is a function of science to study the physics of incandescent lamps or gas motors so as to bring the economically most satisfactory devices into the service of the community, it cannot be less important from the standpoint of national economics to study scientifically the efficiency of the advertisements in order that the national means may in this industry, too, secure the greatest possible effects. It is only a secondary point that experiments of this kind are of high interest to the theoretical scientist as well. For us the advertisement is simply an instrument constructed to satisfy certain human demands by its effects on the mind. It is a question for psychology to determine the conditions under which this instrument may be best adapted to its purpose.

The mental effect of a well-adapted advertisement is manifold. It appeals to the memory. Whatever we read at the street corner, or in the pages of the newspaper or magazine, is not printed with the idea that we shall immediately turn to

the store, but first of all with the expectation that we keep the content of the advertisement in our memory for a later purchase. It will therefore be the more valuable the more vividly it forces itself on the memory. But if practical books about the art of advertising usually presuppose that this influence on the memory will be proportionate to the effect on the attention, the psychologist cannot fully agree. The advertisement may attract the attention of the reader strongly and yet by its whole structure may be unfit to force on the memory its characteristic content, especially the name of the firm and of the article. The pure memory-value is especially important, as according to a well-known psychological law the pleasure in mere recognition readily attaches itself to the recognized object. The customer who has the choice among various makes and brands in the store may not have any idea how far one is superior to another, but the mere fact that one among them bears a name which has repeatedly approached his consciousness before through advertisements is sufficient to arouse a certain warm feeling of acquaintance, and by a transposition of feeling this pleasurable tone accentuates the attractiveness of that make and leads to its selection. This indirect help through the memory-value is economically no less important than the direct service.

In order to produce a strong effect on memory the advertisement must be easily apprehensible. Psychological laboratory experiments with exact time-measurement of the grasping of various advertisements of the same size for the same article, but in different formulations, demonstrated clearly how much easier or harder the apprehension became through relatively small changes. No mistake in the construction of the advertisement causes so much waste as a grouping which makes the quick apperception difficult. The color, the type, the choice of words, every element, allows an experimental analysis, especially by means of time-measurement. If we determine in thousandths of a second the time needed to recognize the characteristic content of an advertisement, we may discriminate differences which would escape the naïve judgment, and yet which in practical life are of considerable consequence, as the effect of a deficiency is multiplied by the number of readers.

We must insist on the further demand that the advertisement make a vivid impression, so that it may influence the memory through its vividness. Size is naturally the most frequent condition for the increase of vividness, but only the relative size is decisive. The experiment shows that the full-page advertisement in a folio magazine does not

influence the memory more than the full page in a quarto magazine, if the reader is for the time adjusted to the particular size. No less important than the size is the originality and the unusual form, the vivid color, the skillful use of empty spaces, the associative elements, the appeal to humor or to curiosity, to sympathy or to antipathy. Every emotion can help to impress the content of the advertisement on the involuntary memory. Unusual announcements concerning the prices or similar factors move in the same direction.

Together with the question of the apprehension and the vividness of the impression, we must acknowledge the frequency of repetition as an equally important factor. We know from daily life how an indifferent advertisement can force itself on our mind, if it appears daily in the same place in the newspaper or is visible on every street corner. But the psychologically decisive factor here is not the fact of the mere repetition of the impression, but rather the stimulation of the attention which results from the repetition. If we remained simply passive and received the impression the second and third and fourth time with the same indifference with which we noticed it the first time, the mere summation would not be sufficient for a strong effect. But the second

impression awakes the consciousness of recognition, thus exciting the attention, and through it we now turn actively to the repeated impression which forces itself on our memory with increased vividness on account of this active personal reaction.

We may consider how such factors can be tested by the psychotechnical experiment. Scott, for instance, studied the direct influence of the relative size of the advertisements.[50] He constructed a book of a hundred pages from advertisements which had been cut from various magazines and which referred to many different articles. Fifty persons who did not know anything about the purpose of the experiment had to glance over the pages of the book as they would look though the advertising parts of a monthly. The time which they used for it was about ten minutes. As soon as they had gone through the hundred pages, they were asked to write down what they remembered. The result from this method was that the 50 persons mentioned on an average every full-page advertisement $6\frac{1}{2}$ times, every half-page less than 3 times, every fourth-page a little more than 1 time, and the still smaller advertisements only about $\frac{1}{7}$ time. This series of experiments suggested accordingly that the memory value of a fourth-page advertisement is much

smaller than one fourth of the memory-value of a full-page advertisement, and that of an eighth-page again much smaller than one half of the psychical value of a fourth-page. The customer who pays for one eighth of a page receives not the eighth part, but hardly the twentieth part of the psychical influence which is produced by a full page.

These experiments, which were carried on in various forms, demanded as a natural supplement a study of the effects of repetition in relation to size. This was the object of a series of tests which I carried on recently in the Harvard laboratory. I constructed the following material: 60 sheets of Bristol board in folio size were covered with advertisements which were cut from magazines the size of the "Saturday Evening Post" and the "Ladies' Home Journal." We used advertisements ranging from full-page to twelfth-page in size. Every one of the 6 full-page advertisements which we used occurred only once, each of the 12 half-page advertisements was given 2 times, each of the fourth-page size, 4 times, each of the eighth-page size, 8 times, and each of the twelfth-page size, 12 times. The repetitions were cut from 12 copies of the magazine number. The same advertisement never occurred on the same page; every page, unless it was covered by a full-page adver-

tisement, offered a combination of various announcements. It is evident that by this arrangement every single advertisement occupied the same space, as the 8 times repeated eighth-page advertisement filled a full page too. Thus no one of the 60 announcements which we used was spatially favored above another.

Thirty persons took part in the experiment. Each one had to devote himself to the 60 pages in such a way that every page was looked at for exactly 20 seconds. Between each two pages was a pause of 3 seconds, sufficient to allow one sheet to be laid aside and the next to be grasped. In 23 minutes the whole series had been gone through, and immediately after that every one had to write down what he remembered, both the names of the firms and the article announced. In the cases where only the name or only the article was correctly remembered, the result counted $\frac{1}{2}$. We found great individual differences, probably not only because the memory of the different persons was different, but also because they varied in the degree of interest with which they looked at such material. The smallest number of reproductions was 18, of which 14 were only half remembered, that is, only the name or only the article, and as we counted these half reproductions $\frac{1}{2}$, the memory-value for this person was counted 11. The

maximum reproduction was 46, of which 6 were half remembered.

If these calculated values are added and the sum divided by the number of participants, that is, 30, and this finally by the number of the advertisements shown, that is, 60, we obtain the average memory-value of a single advertisement. The results showed that this was 0.44. But our real interest referred to the distribution for the advertisements of different size. If we make the same calculation, not for the totality of the advertisements but for those of a particular size, we find that the memory-value for the full-page advertisement was 0.33, for the 2 times repeated half-page advertisement, 0.30, for the 4 times repeated fourth-page advertisement 0.49, for the 8 times repeated eighth-page advertisement, 0.44, and for the 12 times repeated twelfth-page advertisement, 0.47. Hence we come to the result that the 4 times repeated fourth-page advertisement as $1\frac{1}{2}$ times stronger memory-value than one offering of a full-page, or the 2 times repeated half-page, but that this relation does not grow with a further reduction of the size. Two thirds of the subjects were men and one third women. On the whole, the same relation exists for both groups, but the climax of psychical efficiency was reached in the case of the men by the 4 times repeated

fourth-page, in the case of the women by the 8 times repeated eighth-page. The 4 times repeated fourth-page in the case of the women was 0.45, in the case of the men, 0.51, the 8 times repeated eighth-page, women, 0.53, men, 0.37.

I am inclined to believe that the ascent of the curve of the memory-value from the full-page to the fourth-page or eighth-page would have been still more continuous, if the whole-page advertisements had not naturally been such as are best known to the American reader. The whole-page announcement, therefore, had a certain natural advantage. But when we come to another calculation, even the effect of this advantage is lost. We examined the relations for the first 10 names and articles, which every one of the 30 persons wrote down. These first 10 were mostly dashed down quickly without special thought. They also included only a few half reproductions. When we study these 300 answers which the 30 persons wrote as their first 10 reproductions, and calculate from them the chances which every one of the 60 advertisements had for being remembered, we obtain the following values: The probability of being remembered among the first 10 was for the full-page advertisement, 0.5, for the half-page 2 times repeated, 1.2, for the fourth-page 4 times repeated, 2.9, for the eighth-page 8 times repeated,

2.3, and for the twelfth-page 12 times repeated, 2.4. The superiority of repetition over mere size appears most impressively in this form, but we see again in this series that the effect decreases even with increased number of repetitions as soon as the single advertisement sinks below a certain relative size, so that the 12 times repeated twelfth-page advertisement does not possess the memory-value of the 4 times repeated fourth-page advertisement. If Scott's experiments concerning the size and these experiments of mine concerning the repetition are right, the memory-value of the advertisements for economic purposes is dependent upon complicated conditions. A business man who brings out a full-page advertisement once in a paper which has 100,000 readers would leave the desired memory-impression on a larger number of individuals than if he were to print a fourth-page advertisement in four different cities in four local papers, each of which has 100,000 readers. But if he uses the same paper in one town, he would produce a much greater effect by printing a fourth of a page four times than by using a full-page advertisement once only.

As a matter of course this would hold true only as far as size and repetition are concerned. Many other factors have to be considered besides. Some of these could even be studied with our material.

We could study from our results what memory-value is attached to the various forms of type or suggestive words, what influence to illustrations, how far they reinforce the impressiveness and how far they draw away the attention from the name and the object, how these various factors influence men and women differently, and so on. Other questions, however, demand entirely different forms of experiment. We may examine the effects of special contrast phenomena, of unusual background, of irregular borders and original headings. The particular position of the advertisement also deserves our psychological interest. The magazines receive higher prices for the cover pages and the newspapers for advertisements which are surrounded by reading matter. In both cases obvious practical motives are decisive. The cover page comes into the field of vision more frequently. What is surrounded by reading matter is less easily overlooked.

But the newspaper world hardly realizes how much other variations of position influence the psychological effect. Starch[51] made experiments in which he did not use real advertisements, but meaningless syllables so as to exclude the influence of familiarity with any announcement. He arranged little booklets, each of 12 pages, on which a syllable such as *lod*, *zan*, *mep*, *dut*, *yib*,

and so on was printed in the middle of each page. Each of his 50 subjects glanced over the book and then wrote down what syllables remained in memory. He found that the syllables which stood on the first and last page were remembered by 34 persons, those on the second and eleventh by about 26, and those on the eight other pages by an average of 17 persons. In the next experiment he printed one syllable in the middle of the upper and one in the middle of the lower half of each page. The results now showed that of those syllables which were remembered 54 per cent stood on the upper half and 46 per cent on the lower half of the page. Finally, he divided every page into four parts and printed one syllable on the middle of each fourth of a page. The results showed that of the remembered syllables 28 per cent stood on the left-hand upper fourth, 33 per cent on the right-hand upper fourth, 16 per cent left-hand lower, and 23 per cent right-hand lower. A fourth-page advertisement which is printed on the outer side of the upper half of the page thus probably has more than twice the psychological value of one which is printed on the inner side of the lower half. The economic world spends millions every year for advertisements on the upper right-hand side and millions for advertisements on the lower left-hand side, and is not aware that one represents twice

the value of the other. These little illustrations of advertisement experiments may suffice to indicate how much haphazard methods are still prevalent in the whole field of economic psychotechnics, methods which would not be tolerated in the sphere of physical and chemical technology.

XXI

THE EFFECT OF DISPLAY

IF we turn from the simple newspaper advertisement to the means of propaganda in general, we at once stand before a question which is often wrongly answered. The practical handbooks of advertisements and means of display treat it as a self-evident fact that every presentation should be as beautiful as possible. In the first place, we cannot deny that the ugly and even the disgusting possess a strong power for attracting attention. Yet it is true that by a transposition of feelings the displeasure in the advertisement may easily become a displeasure in the advertised object. But, on the other hand, it is surely a mistake to believe that pure beauty best fulfills the function of the advertisement. Even the draftsman who draws a poster ought to give up the ambition to create a perfect picture. It might have the power to attract attention, but it would hardly serve its true purpose of fixing the attention on the article which is advertised by the picture. The very meaning of beauty lies in its self-completeness. The beautiful picture rests in itself and does not point beyond itself. A really

beautiful landscape painting is an end in itself, and must not stir up the practical wish to visit the landscape which has stimulated the eye of the painter. If the display is to serve economic interests, every line and every curve, every form and every color, must be subordinated to the task of leading to a practical resolution, and to an action, and yet this is exactly the opposite of the meaning of art. Art must inhibit action, if it is perfect. The artist is not to make us believe that we deal with a real object which suggests a practical attitude. The æsthetic forms are adjusted to the main æsthetic aim, the inhibition of practical desires. The display must be pleasant, tasteful, harmonious, and suggestive, but should not be beautiful, if it is to fulfill its purpose in the fullest sense. It loses its economic value, if by its artistic quality it oversteps the boundaries of that middle region of arts and crafts. This of course stands in no contradiction to the requirement that the advertised article should be made to appear as beautiful as possible. The presentation of something beautiful is not necessarily a beautiful presentation, just as a perfectly beautiful picture need not have something beautiful as its content. A perfect painting may be the picture of a most ugly person.

We have not yet spoken of the suggestive

power of the means of propaganda. Every one knows how the influence on taste and smell, on social vanity, on local pride, on the gambling instinct, on the instinctive fear of diseases, and above all on the sexual instinct, can gain suggestive power. Everywhere among the uncritical masses such appeals reach individuals whose psychophysical attitudes make such influences vivid and overpowering. Every one knows, too, those often clever linguistic forms which are to aid the suggestion. They are to inhibit the opposing impulses. The mere use of the imperative, to be sure, has gradually become an ineffective, used-up pattern. It is a question for special economic psychotechnics to investigate how the suggestive strength of a form can be reinforced or weakened by various secondary influences. What influence, for example, belongs to the electric sign advertisements in which the sudden change from light to darkness produces strong psychophysical effects, and what value belongs to moving parts in the picture?

The psychologist takes the same interest in the examples of window displays, sample distributions, and similar vehicles of commerce by which the offered articles themselves and not their mere picture or description are to influence the consciousness of the prospective customer.

Here, too, every element may be isolated and may be brought under psychotechnical rules. The most external question would refer to the mere quantity of the presented material. The psychologist would ask how the mere mass of the offering influences the attention, how far the feeling of pleasure in the fullness, how far the æsthetic impression of repetition, how far the associative thought of a manifold selection, how far the mere spatial expansion, affects the impression. In any case, as soon as it is acknowledged as desirable to produce with certain objects the impression of the greatest possible number, the experimental psychologist stands before the concrete problem of how a manifoldness of things is to be distributed so that it will not be underestimated, perhaps even overestimated as to quantity. Again, the laboratory experiment would not proceed with real window displays or real exhibitions, but would work out the principle with the simplified experimental means.

An investigation in the Harvard laboratory, for instance, tested the influence which various factors have upon the estimation of a number of objects seen.[52] The question was how far the form or the size or the distribution makes a group of objects appear larger or smaller. The experiment was started by showing 20 small cards

on a black background in comparison with another group of cards the number of which varied between 17 and 23. At first the form of these little cards was changed: triangles, squares, and circles were tried. Or the color was changed: light and dark, saturated and unsaturated colors were used. Or the order was varied: sometimes the little cards lay in regular rows, sometimes in close clusters, sometimes widely distributed, sometimes in quite irregular fashion. Or the background was changed, or the surrounding frame, or the time of exposure, and so on. Each time the subjects had to estimate whether the second group was the larger or equal or the smaller. These experiments indicated that such comparative estimation was indeed influenced by every one of the factors mentioned. If the experiments show that an irregular distribution makes the number appear larger or a close clustering reduces the apparent number, and so on, the business man would be quite able to profit from such knowledge. The jeweler who shows his rings and watches in his window wishes to produce with his small stock the impression of an ample supply. He lacks the psychology which might teach him whether he would act more wisely in having the rings and the watches separated, or whether he should mix the two, whether he ought to choose

a background which is similar in color or one which contrasts with the pieces exhibited, whether he ought to present the single object in a special background as in a case, or to show it without one. He is not aware that by simple psychological illusions, it is not difficult to change the apparent size of an isolated object by special treatment, making his show-piece appear larger by a fitting background or intentionally making a dainty object appear smaller by contrasting surroundings. These, to be sure, are very trivial illustrations, but the same fundamental psychological laws which are true for the show-window of the next corner store are true for the world-display of the nation. The point is to present clearly the idea, which can be most simply expressed in such trivial material. But it may be added that even in the case of the most indifferent example a few hasty experiments with one or two subjects cannot yield any results of value.

All parts of physiological psychological optics can contribute similar material. The questions of color harmony and color contrast, light intensity and mutual support of uniformly colored objects, of irradiation, depth and perspective, are significant for an effective display in the show-window, and the laboratory results can easily be translated into psychotechnical prescriptions.

But here it is still more necessary to separate carefully the merely optical aspect of the impression from its æsthetic side. All that we claimed as to the poster is still more justified for the presentation of the saleable objects themselves. As soon as the display of the articles forms a real work of art, it must produce inhibitions in the soul of the spectator by which the practical economic desire is turned aside. Beauty here too has strong power of attraction, and moreover the suggestive power, by which it withdraws our senses from the chance surroundings, forces us to lose ourselves in the offered presentation. But just through this process the content of the display becomes isolated and separated from the world of our practical interests. Our desires are brought to silence, we do not seek a personal relation to the things which we face as admiring spectators, and the intended economic effect is therefore eliminated. Whoever is to examine the psychotechnics of displays and exhibitions must therefore study the psychology of æsthetic stimulation, of suggestion, of the effects of light, color, form and movement, of apperception and attention, and ought not to forget the psychology of humor and curiosity, of instincts and emotions. For us the essential point is that here too the experimental psychological method alone is able to lead from

mere chance arrangements founded on personal taste to the systematic construction which secures with the greatest possible certainty the greatest possible mental effect in the service of the economic purpose.

The problems of the storekeeper who arranges his windows, however, overlap the problems of the manufacturer who prepares his goods for the world-market, and who must from the start take care that the outer appearance of his goods stimulate the readiness to buy. In factories in which these questions have been carefully considered, the psychological elements have always been found to be the most influential, but often the most puzzling. I received material from a number of industrial plants which sold the same article in a variety of packings. The material which was sent to me included all kinds of soaps and candies, writing-papers and breakfast foods, and other articles which are handled by the retailer, the sale of which depends upon the inclination and caprice of the customer in the store. For every one of these objects a number of external covers and labels were sent and with them a confidential report with details about their relative success. For instance, a certain kind of chocolate was sold under 12 different labels. One of them was highly successful in the whole country, and one

other had made the same article entirely unsaleable. The other 10 could be graded between these extremes. In all 12 cases the covers were decorated with pictures of women with a scenic background. As long as only æsthetic values were considered, all were on nearly the same level, and æsthetically skilled observers repeatedly expressed their preference for some of the unsuccessful pictures over some of the successful ones. But as soon as an internal relation was formed between the pictures and the chocolate, in the one case a mental harmony resulted which had strong suggestive power, in the other case a certain unrest and inner disturbance which necessarily had an inhibiting influence. The picture which was unsuccessful with the sweets would perhaps have been eminently successful for tobacco. From such elementary starting-points, the laboratory experiment might proceed systematically into spheres of economic life hitherto untouched by scientific methods. The psychology of the influence of external forms on the conscious reactions of the masses is so far usually considered only when, as often happens, the most fundamental demands are violated; for instance, when objects which are to give the impression of ease are painted in colors which give a heavy, clumsy appearance, or vice versa, when book-bindings are

lettered in archaic type which makes the reading of the title impossible for a passer-by, and many similar antipsychological absurdities which any stroll through the streets of a modern city forces on us.

XXII

EXPERIMENTS WITH REFERENCE TO ILLEGAL IMITATION

IT is perhaps not without interest to turn into a by-path at this point of our road. All the illustrations which we have picked out so far have referred to strictly economic conditions. But we ought not to forget that these economic problems of commerce and industry are everywhere in contact with legal interests as well. In order to indicate the manifoldness of problems accessible to the experimental method, we may discuss our last question, the question of packing and of labels, in this legal relation too. All the packings, covers, labels, trademarks, and names by which the manufacturer tries to stimulate the attention, the imagination, and the suggestibility of the customer may easily draw a large part of their psychological effectiveness from without, as soon as they imitate the appearance of articles which are well introduced and favored in the market. If the public is familiar with and favorably inclined toward an article on account of its inner values or on account of its being much ad-

vertised, a similar name or a similar packing may offer efficient help to a rival article. The law of course protects the label and the deceiving imitation can be prosecuted. But no law can determine by general conceptions the exact point at which the similarity becomes legally unallowable. This creates a situation which has given rise to endless difficulties in practical life.

If everything were forbidden which by its similarity to an accredited article might lead to a possible confusion in the mind of the quite careless and inattentive customer, any article once in the market would have a monopoly in its line. As soon as a typewriter or an automobile or a pencil or a mineral water existed, no second kind could have access to the market, as with a high degree of carelessness one economic rival may be taken for another, even if the new typewriter or the new pencil has a new form and color and name. On the other side, the purchaser could never have a feeling of security if imitations were considered as still legally justifiable when the difference is so small that it needs an intense mental effort and careful examination of details to notice it.

The result is that the jurisdiction fluctuates between these two extremes in a most alarming way, and this seems to hold true in all countries. In

theory: "There is substantial agreement that infringement occurs when the marks, names, labels, or packings of one trader resemble those of another sufficiently to make it probable that ordinary purchasers, exercising no more care than such persons usually do in purchasing the article in question, will be deceived." But it depends upon the trade experts and the judges to give meaning to such a statement in the particular case, as the amount of care which purchasers usually exercise can be understood very differently. Sometimes the customer is expected to proceed with an attention which is most subtly adjusted to the finest differences, and sometimes it is taken for granted that he is unable to notice even strong variations. It is clear that this uncertainty which disturbs the whole trade cannot be eliminated as long as the psychological background has not been systematically studied. Mere talking about the attention of the customer, and his ability to decide and select, and of his observations and his habits in the spirit of popular common-sense psychology, can never secure exact standards and definite demarcation lines. The question is important not only where imitations of morally doubtful character are in the market. Even the most honest manufacturer is in a certain sense obliged to imitate his predecessors,

as they have directed the taste and habits of the public in particular directions, and as the product of his company would suffer unnecessarily if he were to disregard this psychical attitude of the prospective customers. The economic legal situation accordingly suggests the question whether it would not be possible to devise methods for an exact measurement of the permissible similarity, and this demand for exactitude naturally points to the methods of the psychological experiment. E. S. Rogers, Esq., of Chicago, who has thoroughly discussed the legal aspect of the problem,[53] first turned my attention to the psychological difficulty involved.

When I approached the question in the Harvard psychological laboratory, it was clear to me that the degree of attention and carefulness which the court may presuppose on the part of the customer can never be determined by the psychologist and his experimental methods. It would be meaningless, if we tried to discover by experiments a particular degree of similarity which every one ought to recognize or a particular degree of attention which would be sufficient for protection against fraud. Such degrees must always remain dependent upon arbitrary decision. They are not settled by natural conditions, but

are entirely dependent upon social agreement. A decision outside of the realm of psychology must fix upon a particular degree in the scale of various similarity values as the limit which is not to be passed. The aim of the psychologist can be only to construct such a scale by which decisions may be made comparable and by which standards may become possible. The experiment cannot deduce from the study of mental phenomena what degrees of similarity ought to be still admissible, but it may be able to develop methods by which different degrees of similarity can be discriminated and by which a certain similarity value once selected can always be found again with objective certainty. After many fruitless efforts I settled on the following form of experiments, which I hope may bring us nearer to the attainment of the purpose.

A group of objects is observed for a definite time and after a definite interval another group of objects is offered for comparison. This second group is identical with the first in all but one of the objects, and this is replaced by a similar one. The question is how often this substitution will be noticed by the observers. I may give in detail a characterization of the set of experiments in which we are at present engaged. We are working with picture postal cards, using many hun-

dred cards of different kinds, but for each one we have one or several similar cards. As postal cards are generally manufactured in sets, it is not difficult to purchase pairs of pictures with any degree of similarity. Two cards with Christmas trees, or two with Easter eggs, or two with football players, or two with forest landscapes, and so on, may differ all the way from a slight variation of color or a hardly noticeable change in the position of details to variations which keep the same motive or the same general arrangement, but after all make the card strikingly different. The first step is to determine for each pair the degree of similarity, on a percentage basis. To overcome mere arbitrariness, we ask thirty to forty educated persons to express the similarity value, calling identical postal cards 100 per cent and two postal cards as different as a colored flower piece and a black picture of a street scene 0. The average value of these judgments is then considered as expressing the objective degree of similarity between the two pictures of a pair. After securing such standard values, we carry on the experiments in the following form. Six different postal cards, for instance, are seen on a black background through the opening of a shutter which is closed after 5 seconds. The six may be made up of a landscape, a building, a head, a genre scene, and

so forth. After 20 seconds the same group of postal cards is shown once more, except that one is replaced by a similar one, instead of one church another church building, or instead of a vase with roses a vase with pinks. If the substituted picture has the average similarity value of 80 per cent and we make the experiment with 10 persons, the substitution may be discovered by 7 persons and remain unnoticed by 3. We can now easily vary every one of the factors involved. If instead of 6 cards, we take 10, it may be that only 4 out of 10 persons, instead of 7, will discover the substitution, while if we take 4 cards instead of 6, perhaps 9 persons out of 10 will recognize the difference under these otherwise equal conditions. Only an especially careless observer will overlook it. But instead of changing the number of objects, we may change the periods of exposure. If we show the 6 cards only for 2 seconds instead of 5 seconds, the number of those who recognize the difference may sink from 7 to 5 or 4, and if we make the time considerably longer, we shall of course reach a point where all 10 will recognize the substitution. The same holds true of the shortening or lengthening of the time-interval between the two presentations. The third variable factor is the similarity itself. If instead of one church, not another church, but a theatre or a skyscraper is

shown, that is, if the similarity value of 80 per cent sinks down to a similarity of 60 per cent or 50 per cent, the number of those who recognize the substitution will again become larger; if, on the other hand, the substituted card shows the same church, only from a slightly different angle, bringing the similarity value up to 90 per cent or 95 per cent, the number of observers who recognize the substitution may sink to 2 or 3. To make the experiments reliable, it is also necessary frequently to mix in cases in which no substitution at all is introduced.

If these experiments are varied sufficiently and a large mass of material brought together, we must be able to secure definite formulæ. We may find that if the critical card appears among 6 cards, is shown for 5 seconds, and the group is again exposed after 20 seconds, 80 per cent of the subjects will recognize the substitution of a similar card, if the degree of similarity is 30 per cent, but only 60 per cent will recognize it if the degree of similarity is 70 per cent, and only 30 per cent will recognize it if the degree of similarity is 90 per cent. These are entirely fictitious figures and are only to indicate the principle. If such an exact formula were definitely discovered, we should still be unable to say from mere psychological reasoning what similarity value is legally permissible. If

the rules against infringement are interpreted in a very rigorous spirit, it may seem desirable to prohibit imitations which are as little similar as those postal cards which were graded as 40 per cent in our similarity scale, and if the interpretation is a loose one, it may appear permissible to have imitations on the market which are as strongly similar as our postal cards graded at 80 per cent in our similarity scale. All this would have to be left to the lawmakers and to the judges. But what we would have gained is this. We could say: if our object exposed for 5 seconds in a group of 6 other objects is replaced after an interval of 20 seconds by an imitation and this change is recognized by 8 persons among 10, the degree of similarity is 30 per cent and if it is recognized by 3 out of 10 subjects, the degree of similarity is 90 per cent. In short, from any percentage of subjects who under these conditions discovered the substitution, we could determine the degree of similarity, independent of any individual arbitrariness. If such methods were accepted by the trade and the courts, it would only be necessary to agree on the percentage of similarity which ought to be permitted, and all uncertainty would disappear. There would be no wrangling of opposing interests; it would be possible to find out whether the permitted limit were overstepped or

not with an exactitude similar to that with which the weight or the chemical constitution of a trade commodity is examined. Certainly the experiment establishes here conditions which are very different from those of practical life. The customer who wants to buy a particular picture postal card which he saw once before and to whom the salesman offers a similar one, suggesting that it is the same, is facing only one card and not a group of six. But in practical life the card which he has seen was not observed with the definite intention of keeping the memory picture in mind, and months may have passed since it was seen. The memory picture which the customer has in his consciousness when he seeks the particular card is much weakened by this circumstance too. We secure this weakening artificially by the arrangement of the experiment in placing the card in a group of six or ten and exposing them for a few seconds only. The force of attention and the corresponding memory-value are by this distribution diminished in a definite degree in the case of every single card.

The investigation must include a careful study of the size of the groups, of the time-relations, of the percentage of correct answers, all under the point of view of greatest fitness for practical application. In the Harvard laboratory the research

has been carried on partly with such picture material, partly with word material, and partly with concrete objects.[54] Whatever the details of the outcome may be, we hope that the work will lead to results which may, indeed, make such a psychotechnical use possible. Its principles and formulæ might easily be adjusted to any marketable material. As a matter of course, if in future the courts were ever to accept such psychological, experimental methods, it would be intolerable dilettantism if such experiments were carried on by lawyers and district attorneys. It is as true of this economic legal question as of many other legal psychological problems that its introduction into the courtroom can become desirable only when psychological experts are engaged and called in the same way as chemical or medical experts are invited to the court. On the other hand, there is surely not the slightest desire on the part of psychologists to be dragged into humiliating performances like those which not only handwriting experts, but even psychiatric specialists have had to undergo repeatedly in sensational court trials. The day for the expert activity in the courtroom will come for the psychologist only when the country has attached the expert to the court and has eliminated the expert retained by the plaintiff or the defendant. But this general

practical question as to the position of the psychologist in the courtroom and as to the need of a psychological laboratory in connection with the courts would lead us too far aside.

XXIII

BUYING AND SELLING

THE effects which we have studied so far were produced by inanimate objects, posters or displays, advertisements or labels and packings. The economic psychotechnics of the future will surely study with similar methods the effects of the living commercial agencies. Experiments will trace the exact effects which the salesman or customer may produce. But here not even a modest beginning can be discovered, and it would be difficult to mention a single example of experimental research. The desired psychological influences of the salesman are not quite dissimilar to those of the printed means of propaganda. Here, too, it is essential to turn the attention of the customer to different points, to awaken a vivid favorable impression, to emphasize the advantages of the goods, to throw full light on them, and finally to influence the will-decision either by convincing arguments or by persuasion and suggestion. In either case the point is to enhance the impulse to buy and to suppress the opposing ideas. Yet every one of these factors, when it starts from a man and not from a thing or paper, changes its

form. The influence becomes narrower, it is directed toward a smaller number of persons; but, on the other hand, it gains just by the new possibility of individualization. The salesman in the store or the commercial traveler adjusts himself to the wishes, reactions, and replies of the buyer. Above all, when it becomes necessary to direct the attention to the decisive points, the personal agent has the possibility of developing the whole process through a series of stages so that the attention slowly becomes focused on one definite point. The salesman observes at first only the general limits of the interest of the customer as far as it is indicated by his reactions, but slowly he can find out in this whole field the region of strongest desires. As soon as he has discovered this narrower region in which the prospects of success seem to be greatest, he can systematically eliminate everything which distracts and scatters the attention. He can discover whether the psyche of the individual with whom he is dealing can be influenced more strongly by logical arguments or by suggestion, and how far he may calculate on the pleasure instincts, on the excitement of emotions, on the impulse to imitate, on the natural vanity, on the desire for saving, and on the longing for luxury. In every one of these directions the whole play of human suggestion may be helpful.

The voice may win or destroy confidence, the statement may by its firmness overcome counter-motives or by its uncertainty reinforce them. Even hand or arm movements by their motor suggestion may focus the desires of the customers, while unskillful, erratic movements may scatter the attention and lead to an inner oscillation of the will to buy.

At every one of these points the psychological experiment may find a foothold, and only through such methodological study can the haphazard proceedings of the commercial world be transformed into really economic schemes. Indeed, it seems nothing but chance that just this field is controlled by chance alone. The enormous social interplay of energies which are discharged in the selling and buying of the millions becomes utterly planless as soon as salesman and customer come into contact, and this tremendous waste of energy cannot appear desirable for any possible interest of civilization. The time alone which is wasted by useless psychophysical operations in front of and behind the counter represents a gigantic part of the national budget. Even the complaints about the long working day of the salesgirls might be eliminated from the debit account of the national ledger, if the commercial companies could study the psychical processes in

selling and buying with the same carefulness with which they analyze all details in preparing the stock and fixing the prices. In the army or in the fire department, in the railroad service, and even in the factory, all necessary activities are so arranged that as far as possible the greatest achievement is secured by the smallest amount of energy. But when the hundreds of millions of customers in the civilized world want to satisfy their economic demands in the stores, the whole dissolves into a flood of talk, because no one has taken the trouble to examine scientifically the psychotechnics of selling and to put it on a firm psychological foundation.

The idea of scientific management must be extended from the industrial concerns to the commercial establishments. The questioning and answering, the showing and replacing of the goods, the demonstrating and suggesting by the salesmen, must be brought into an economic system which saves time and energy, as has been tried with the laborer in the factory. Wherever economic processes are carried out with superfluous, haphazard movements, the national resources have to suffer a loss. The single individual can never find the ideal form of motion and the ideal process by mere instinct. A systematic investigation is needed to determine the way to the great-

est saving of energy, and the result ought to be made a binding rule for every apprentice. How the smallest influences grow by summation may be illustrated by the experience of a large department store, in which the expense for delivery of the articles sold was felt as too large an item in the budget. The hundreds of saleswomen therefore received the order after every sale of moderate-sized articles not to ask, as before, "May we send it to you?" but instead, "Will you take it with you?" Probably none of the many thousand daily customers observed the difference, the more as it was indifferent to most of them whether they took the little package home themselves or not. In cases in which it was inconvenient, they would anyhow oppose the suggestion and insist that the purchase be sent to them. Yet it is claimed that this hardly noticeable suggestion led to a considerable saving in the following year, distinctly felt in the budget of the whole establishment.

We must not forget, however, that the process of buying deserves the same psychological interest as that of selling. If psychotechnics is to be put into the service of a valuable economic task, the goal cannot possibly be to devise schemes by which the customer may easily be trapped. The purpose of science cannot be to help any one to

sell articles to a man who does not need them and who would regret the purchase after quiet thought. The applied psychologist should help the prospective buyer no less, and must protect him so that his true intention may become realized in the economic process. Otherwise through his suggestibility, the determining idea of his goal might fade in his consciousness and the appeal to his vanity or to his instincts might awaken an anti-economic desire which he would be too weak to inhibit. The salesman must know how to use arguments and suggestions and how to make them effective,[55] but the customer too must know how to see through a misleading argument and how to resist mere suggestion.

The postulate that the psychical factors in commercial life are to be carefully regarded is repeated in more complex form in the wholesale business and in the stock exchange. It is a perfectly justified and consistent thought which recently led a large credit bureau to an effort to base its information on psychological analysis. It is well known that there are bureaus in which the ledger experiences of a large circle of companies in the same commercial line are collected, tabulated, and recorded, thus affording an automatic review of the occurrences, focusing early attention on doubtful accounts and pointing out weaknesses

in the customers' conditions, as they develop, as well as evidences of prosperity. The ledger experience which a single company has with all its customers is tabulated without revealing its identity to the associates, who get reports containing it, and the many combined ledgers become a valuable guide. Yet all such methods can show only actual movements in the market, and cannot allow the prospects of future development to be determined, simply because they cannot take into account the personal equations. Only an acquaintance with the character and the temperament, the intelligence and the habits, the energy and the weakness, of the head of a firm can tell us whether the company, even with satisfactory resources, may go down, or whether, even though embarrassed, it may hold out. The psychological pioneer, therefore, aims not only toward an exchange of ledger accounts, but toward a real psychological diagnosis and prognosis. If a member of a firm is personally known to some scores of business men who have had commercial dealings with him, and each one of them, without disclosing his identity to any one but the central bureau, sends to it a statement of personal impressions, a composite picture of the mental physiognomy can be worked out. Of course all this has been often done in the terms of popular psychology

and in a haphazard, amateurish way. The new plan is to arrange the questions systematically under the point of view of scientific descriptive psychology. Regular psychograms, in which the probability of a particular kind of behavior is to be determined in an exact percentage calculation, are to replace the traditional vagueness, as soon as a sufficient number of reliable answers have been tabulated.

Commercial life as a whole finds its contact with psychology, of course, not only in the problem of how to secure the best mental effect. Those other questions which we have discussed essentially with reference to factory life and industrial concerns, namely, how the best man and the best work are to be secured, recur in the circle of commercial endeavors. It seems, indeed, most desirable to devise psychological tests by which the ability to be a successful salesman or saleswoman may be determined at an early stage. The lamentable shifting of the employees in all commercial spheres, with its injurious social consequences, would then be unnecessary, and both employers and employees would profit. Moreover, like the selection of the men, the means of securing the most satisfactory work from them, has also so far been left entirely to common sense. Commercial work stands under an abundance

of varying conditions, and each may have influences the isolated effects of which are not known, because they have not been studied in that systematic form which only the experiment can establish. The popular literature on this whole group of subjects is extensive, and in its expansion corresponds to the widespread demand for real information and advice to the salesman. But hardly any part of the literature in the borderland regions of economics is so disappointing in its vagueness, emptiness, and helplessness. Experimental psychology has nothing with which to replace it to-day, but it can at least show the direction from which decisive help may be expected in future.

XXIV

THE FUTURE DEVELOPMENT OF ECONOMIC PSYCHOLOGY

HERE we may stop. From those elementary questions concerning the mental effects, the path would quickly lead to questions of gravest importance. What is the mental effect which the economic labor produces in the laborer himself? How do economic movements influence the mind of the community? How far do non-economic factors produce effects on the psychical mechanism of the economic agents? But it would be idle to claim to-day for exact psychology, with its methods of causal thought, regions in which so far popular psychology, with its methods of purposive thought, is still sovereign. Our aim certainly was not to review the totality of possible problems related to economic efficiency, but merely to demonstrate the principles and the methods of experimental economic psychology by a few characteristic illustrations. As all the examples which we selected were chosen only in order to make clear the characteristic point of view of psychotechnics, it is unimportant whether the particular results will stand the test of further

experimental investigations, or will have to be modified by new researches. What is needed to-day is not to distribute the results so far reached as if they were parts of a definite knowledge, but only to emphasize that the little which has been accomplished should encourage continuous effort. To stimulate such further work is the only purpose of this sketch.

This further work will have to be a work of co-operation. The nature of this problem demands a relatively large number of persons for the experimental treatment. With most experimental researches in our psychological laboratories, the number of the subjects experimented on is not so important as the number of experiments made with a few well-trained participants. But with the questions of applied psychology the number of persons plays a much more significant rôle, as the individual differences become of greatest importance. The same problems ought therefore to be studied in various places, so that the results may be exchanged and compared. Moreover, these psychological economic investigations naturally lead beyond the possibilities of the university laboratories. To a certain degree this was true of other parts of applied psychology as well. Educational and medical experimental psychology could not reach their fullest productivity until

the experiment was systematically carried into the schoolroom and the psychiatric clinic. But the classroom and the hospital are relatively accessible places for the scientific worker, as both are anyhow conducted under a scientific point of view. The teacher and the physician can easily learn to perform valuable experiments with school children or with patients. This favorable condition is lacking in the workshop and the factory, in the banks and the markets. The academic psychologist will be able to undertake work there only with a very disturbing expenditure of time and only under exceptional conditions. If such experiments, for instance, with laborers in a factory or employees of a railway are to advance beyond the faint first efforts of to-day and are really to become serviceable to the cultural progress of our time with effective completeness, they ought not to remain an accidental appendix to the theoretical laboratories. Either the universities must create special laboratories for applied psychology or independent research institutes must be founded which attack the new concrete problems under the point of view of national political economy. Experimental workshops could be created which are really adjusted to the special practical needs and to which a sufficiently large number of persons could be

drawn for the systematic researches. The ideal solution for the United States would be a governmental bureau for applied psychology, with special reference to the psychology of commerce and industry, similar to the model agricultural stations all over the land under the Department of Agriculture.

Only when such a broad foundation has been secured will the time be ripe to carry the method systematically into the daily work. The aim will never be for real experimental researches to be performed by the foreman in the workshop or by the superintendent in the factory. But slowly a certain acknowledged system of rules and prescriptions may be worked out which may be used as patterns, and which will not presuppose any scientific knowledge, any more than an understanding of the principles of electricity is necessary for one who uses the telephone. But besides the rigid rules which any one may apply, particular prescriptions will be needed fitting the special situation. This leads to the demand for the large establishments to appoint professionally trained psychologists who will devote their services to the psychological problems of the special industrial plant. There are many factories that have scores of scientifically trained chemists or physicists at work, but who would

consider it an unproductive luxury to appoint a scientifically schooled experimental psychologist to their staff. And yet his observations and researches might become economically the most important factor. Similar expectations might be justified for the large department stores and especially for the big transportation companies. In smaller dimensions the same real needs exist in the ordinary workshop and store. It is obvious that the professional consulting psychologist would satisfy these needs most directly, and if such a new group of engineers were to enter into industrial life, very soon a further specialization might be expected. Some of these psychological engineers would devote themselves to the problems of vocational selection and appointment; others would specialize on questions of advertisement and display and propaganda; a third group on problems of fatigue, efficiency, and recreation; a fourth on the psychological demands for the arrangement of the machines; and every day would give rise to new divisions. Such a well-schooled specialist, if he spent a few hours in a workshop or a few days in a factory, could submit propositions which might refer exclusively to the psychological factors and yet which might be more important for the earning and the profit of the establishment than the mere buying of new

machines or the mere increase in the number of laborers.

No one can deny that such a transition must be burdened with difficult complications and even with dangers; and still less will any one doubt that it may be caricatured. One who demands that a chauffeur or a motorman of an electric railway be examined as to his psychical abilities by systematic psychological methods, so that accidents may be avoided, does not necessarily demand that a congressman or a cabinet minister or a candidate for marriage be tested too by psychological laboratory experiments, as the witty ones have proposed. And one who believes that the work in the factory ought to be studied with reference to the smallest possible expenditure of psychical impulses is not convinced that the same experimental methods will be necessary for the functions of eating and drinking and love-making, as has been suggested.

And if it is true that difficulties and discomforts are to be feared during the transition period, they should be more than outweighed by the splendid betterments to be hoped for. We must not forget that the increase of industrial efficiency by future psychological adaptation and by improvement of the psychophysical conditions is not only in the interest of the employers, but still more of the

employees; their working time can be reduced, their wages increased, their level of life raised. And above all, still more important than the naked commercial profit on both sides, is the cultural gain which will come to the total economic life of the nation, as soon as every one can be brought to the place where his best energies may be unfolded and his greatest personal satisfaction secured. The economic experimental psychology offers no more inspiring idea than this adjustment of work and psyche by which mental dissatisfaction in the work, mental depression and discouragement, may be replaced in our social community by overflowing joy and perfect inner harmony.

THE END

NOTES

3'11

NOTES

[1] (p. 10). The fullest account of the modern studies on individual differences is to be found in: William Stern: Die differentielle Psychologie in ihren methodischen Grundlagen. (Leipzig, 1911.)

[2] (p. 15). The practical applications of psychology in education, law, and medicine, I have discussed in detail in the books: Münsterberg: Psychology and the Teacher. (New York, 1910.) Münsterberg: On the Witness Stand. (New York, 1908.) (English edition under the title: Psychology and Crime.) Münsterberg: Psychotherapy. (New York, 1909.)

[3] (p. 41). Frank Parsons: Choosing a Vocation. (Boston, 1909.)

[4] (p. 42). M. Bloomfield: The Vocational Guidance of Youth. (Boston, 1911.)

[5] (p. 47). Vocations for Boys. (Issued by the Vocation Bureau of Boston. 1912.) Vocations for Boston Girls. (Issued by the Girls' Trade Education League. 1911.) Bulletins of Vocation Series. Issued by the Women's Educational and Industrial Union. 1911.)

[6] (p. 49). F. W. Taylor: The Principles of Scientific Management. (New York, 1911.) H. L. Gantt: Work, Wages, and Profits. (New York, 1912.) And the books of Emerson, Gilbreth, Goldmark, etc., to be mentioned later.

[7] (p. 52). H. Emerson: Efficiency as a Basis for Operation and Wages. (New York, 1912, p. 107.)

[8] (p. 52). H. Emerson: The Twelve Principles of Efficiency. (New York, 1912, p. 176.)

[9] (p. 53). H. Emerson: The Twelve Principles, p. 156.

[10] (p. 53). H. Emerson: The Twelve Principles, p. 177.

[11] (p. 54). F. W. Taylor: The Principles of Scientific Management, pp. 86–97.

[12] (p. 86). The experiments are being conducted and will be published by Mr. J. W. Bridges.

[13] (p. 98). Investigation of Telephone Companies: Bureau of Labor. (Washington, Government Printing Office, 1910.)

[14] (p. 104). Ries: Beiträge zur Methodik der Intelligenzprüfung. (Zeitschrift für Psychologie, 1910, vol. 56.)

[15] (p. 112). For a survey of a large number of such tests and bibliography, compare: G. M. Whipple: Manual of Mental and Physical Tests. (Baltimore, 1911.)

[16] (p. 126). F. L. Wells: The Relation of Practice to Individual Differences. (American Journal of Psychology, 1912, vol. 23, pp. 75–88.)

[17] (p. 127). M. Bernays: Auslese und Anpassung der Arbeiterschaft der geschlossenen Grossindustrie, dargestellt an den Verhältnissen der Gladbacher Spinnerei und Weberei. (Leipzig, 1910, p. 337.)

[18] (p. 135). H. C. McComas: Some Types of Attention. (Psychological Review Monographs, vol. 13, 3, 1911.)

[19] (p. 149). Max Weber: Zur Psychophysik der industriellen Arbeit. (Archiv für Sozialwissenschaft und Sozialpolitik, 1908 and 1909, vols. 27 and 28.)

[20] (p. 150). Bryan and Harter: Studies in the Telegraphic Language. (Psychological Review, vol. 4.)

[21] (p. 151). W. F. Book: The Psychology of Skill. (University of Montana, Publications in Psychology, 1910.)

[22] (p. 156). H. Münsterberg: Beiträge zur experimentellen Psychologie. (Book IV, 1892.)

[23] (p. 156). A. J. Culler: Interference and Adaptability. (Archives of Psychology, 1912.)

[24] (p. 157). The experiments are being conducted and will be published by Mr. L. W. Kline.

[25] (p. 160). Adolf Gerson: Die physiologischen Grundlagen der Arbeitsteilung. (Zeitschrift für Sozialwissenschaft, 1907.)

[26] (p. 163). Karl Bücher: Arbeit und Rhythmus. (Fourth Edition, Leipzig, 1909, p. 438.)

[27] (p. 164). Frank G. Gilbreth: Motion Study. (New York, 1911.)

[28] (p. 166). Taylor: The Principles of Scientific Management. (New York, 1911, p. 71.)

[29] (p. 169). The experiments are being conducted and will be published by Mr. E. R. Riesen.

[30] (p. 170). R. Herbertz: Zur Psychologie des Maschinenschreibens. (Zeitschrift für angewandte Psychologie, 1908, vol. 2, p. 551.)

[31] (p. 175). C. L. Vaughan: The Moter Power of Complexity. (Harvard Psychological Studies, vol. 2, 1906, p. 527.)

[32] (p. 177). G. M. Stratton: Some Experiments in the Perception of the Movement, Color, and Direction of Lights. (Psychological Review Monographs, vol. 10, 1908.)

[33] (p. 178). The experiments are being conducted and will be published by Miss L. M. Seeley.

[34] (p. 187). H. Münsterberg: Beiträge zur experimentellen Psychologie. (Book IV, 1892.)

[35] (p. 187). R. S. Woodworth: Accuracy of Voluntary Movement. (Psychological Review Monographs, vol. 3, 1899.)

[36] (p. 188). B. A. Lenfest: The Accuracy of Linear Movement. (Harvard Psychological Studies, vol. 2, 1906.)

[37] (p. 202). Ranschburg: Ueber die Bedeutung der Ähnlichkeit beim Erlernen, Behalten und bei der Reproduktion. (Journal für Psychologie und Neurologie, vol. 5.) Ranschburg: Zeitschrift für Psychologie und Physiologie der Sinnesorgane. (Vol. 30, 1902.)

[38] (p. 202). H. Kleinknecht: The Interference of Optical Stimuli. (Harvard Psychological Studies, vol. 2, 1906.)

[39] (p. 205). The experiments are being conducted and will be published by Miss O. E. Martin.

[40] (p. 210). Henry P. Kendall: Unsystematized, Systematized, and Scientific Management. (In Addresses and Discussions at the Conference on Scientific Management held at Dartmouth College, 1912.)

[41] (p. 213). Ernst Abbe: Gesammelte Abhandlungen. (Jena, 1908, vol. 3, p. 206.)

[42] (p. 214). A full survey of the problem and its literature is contained in: Josephine Goldmark: Fatigue and Efficiency. (New York, 1912.)

[43] (p. 217). F. W. Taylor: The Principles of Scientific Management. (New York, 1911, p. 58.)

NOTES

[44] (p. 223). W. Hellpach: Die geopsychischen Erscheinungen, Wetter, Klima und Landschaft und ihr Einfluss auf das Seelenleben. (Leipzig, 1911, p. 176–212.)

[45] (p. 225). Aschaffenburg: Praktische Arbeit unter Alkoholwirkung. (Psychologische Arbeiten — Kraepelin, vol. 1, 1906.)

[46] (p. 228). Hildebrand: Die Beeinflussung der Willenskraft durch den Alkohol. (Königsberg, 1910.)

[47] (p. 232). For the scientific facts concerning alcohol and bibliography compare: Hugo Hoppe: Die Tatsachen über den Alkohol. (Munich, 1912.)

[48] (p. 232). H. L. Hollingworth: The Influence of Caffein on Mental and Motor Efficiency. (New York, 1912.)

[49] (p. 238). Levenstein: Die Arbeiterfrage. (Munich, 1912.)

[50] (p. 263). W. D. Scott: The Psychology of Advertising. (Boston, 1908, p. 166.)

[51] (p. 269). D. Starch: Psychology of Preferred Positions. (Judicious Advertising, New York, November, 1909.)

[52] (p. 275). C. T. Burnett: The Estimation of Number. (Harvard Psychological Studies, vol. 2, p. 349.)

[53] (p. 285). E. S. Rogers: The Unwary Purchaser: A Study in the Psychology of Trade Mark Infringement. (Michigan Law Review, vol. 8, 1910.)

[54] (p. 292). The experiments are being conducted and will be published by Mr. G. A. Feingold.

[55] (p. 299). W. D. Scott: Influencing Men in Business. (New York, 1911.)

INDEX

317

INDEX

INDEX

INDEX

321

v.

The Riverside Press
CAMBRIDGE . MASSACHUSETTS
U . S . A

· 65/

No 9.

SIZE AND FORM PERCEPTION IN *GALLUS DOMESTICUS*

HAROLD C. BINGHAM
Ellsworth College
From the Harvard Psychological Laboratory

Four figures

CONTENTS

I. APPARATUS

A frequent defect appearing in past experiments on visual acuity in animals is the inadequate description of methods. In many cases the conditions have been such that they could not be accurately described. Complexities have been involved making it impossible to ascertain with certainty the visual factors on which the animals have relied in their discriminations. The apparatus used in the observations which are reported in this paper was devised for the purpose of overcoming this difficulty. Controllability was the primary aim in its construction.

To Professor Yerkes, under whom this study was made and to whom I am indebted for assistance and suggestions, credit is due for many of the points of method here described. A report has been published by him and Professor Watson [1] in which the details of parts of the apparatus have been presented. For the detail of divisions *2* and *3* the reader is referred to the Yerkes and Watson report. In order to give a comprehensive idea of the mechanism, I shall describe the complete apparatus as it appeared during my work, abbreviating, as much as possible, descriptions of the parts which have been previously described.

Figure 1 is an isometric view of the apparatus as a whole showing only the skeleton of the different parts when assembled for experimental work. I shall speak of that part of the mechanism labelled I as the *experiment box*. The opposite end, III, is the light or *source box*. Between the experiment box and the source box is a *stimulus shifter*, II. The whole apparatus was set up in a dark room. The only sources of illumination were the lamps in the source box and a lamp, L, hanging directly above the experiment box. With these sources cut off, a dark adapted human eye could see slightly, for a few small cracks in the room allowed faint rays of light to enter; but the subsequent description will show that these factors were of no consequence.

As a means of testing the chicks' ability to discriminate between sizes and forms, two illuminated areas differing from each other with respect to one or both of these factors are simultaneously exposed before the experiment box, I. The problem for the animal is to learn regularly to choose one of these stimuli and to reject the other. It is punished by means of an electric shock for a wrong choice and is rewarded for a right choice by escape to a warm, dry nest box where food, light, water, and companionship are to be found. The illumination of the visual stimuli comes from the source box, III, which is constructed so that the brightness of either stimulus area may be independently controlled. The light is admitted to two of three regulated apertures in the shifter, II, the function of which is to facilitate changes in the size, form, or relative position of the stimulus areas.

---

[1] Yerkes, Robert M. and Watson, John B. Methods of studying vision in animals. *Behavior Monographs*, 1911, vol. 1, no. 2, S. N. 2.

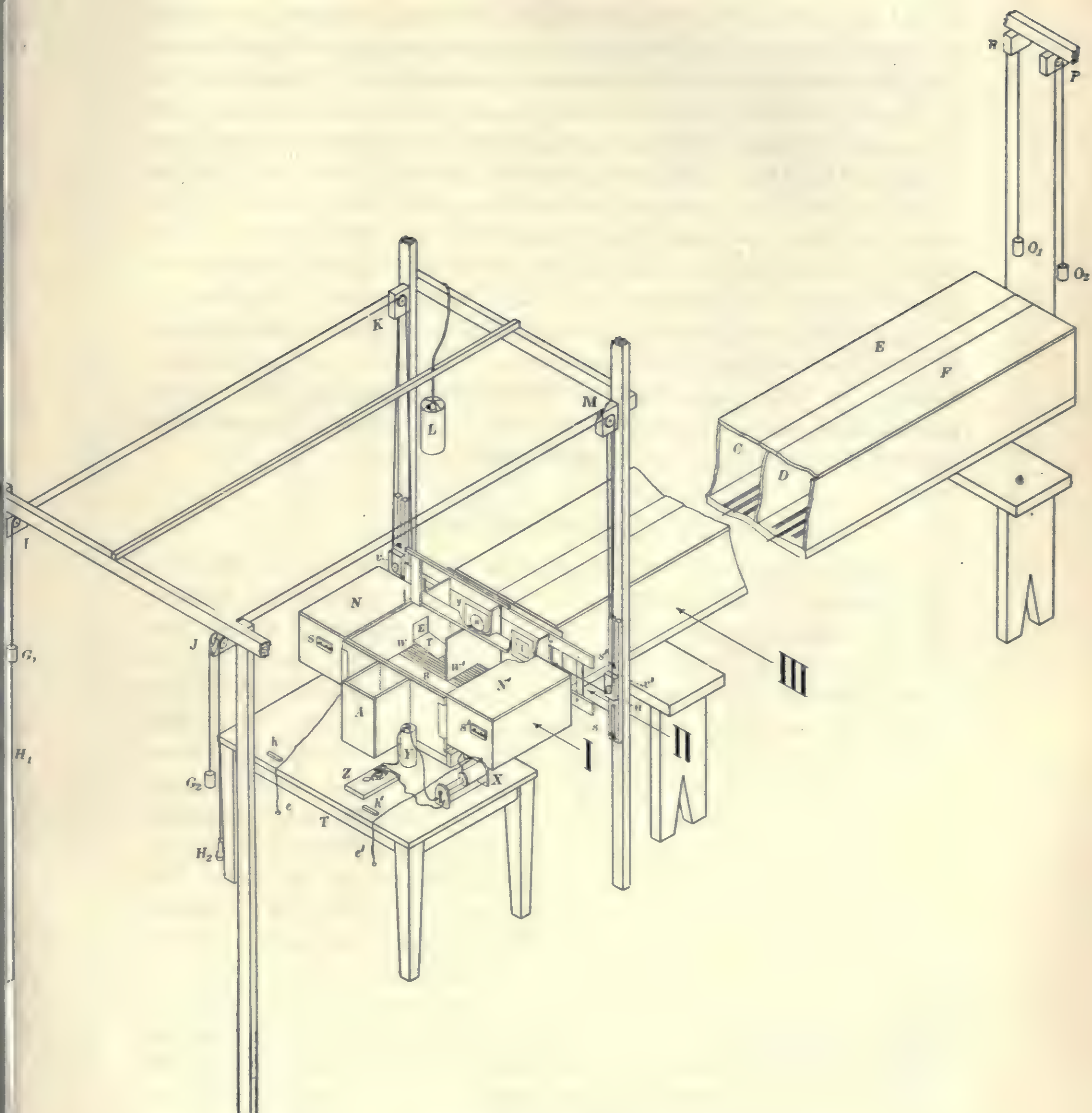


FIGURE 1. Isometric view of *light* vision apparatus.

If the form of the two end stimulus areas is square, and that of the middle one is circular, a movement of the adapter to the left produces the condition shown in figure 1. The stimulus *l*, at the right is a square and that, *n*, at the left is a circle. But when the shifter is moved to the right and the relative positions of the two forms with respect to each other become reversed since the square at the extreme left (now invisible) will take the place of the circle, the circle will move over to the place now occupied by the square, while the present square will be shifted to the extreme right and will become invisible.

This brief account of the apparatus as a whole should make it possible to grasp more easily the details of the various sections. An understanding of the construction and use of the mechanism will be made less difficult if the details are now considered in connection with each general part.

1. Experiment box

An illustration of the experiment box appears as figure 2. This section of the apparatus is made of one-half inch lumber, (except where stated otherwise), and is painted within and without a dead black. It consists of four main parts: (1) A is an entrance chamber, $15\frac{1}{2} \times 20 \times 22$.[2] The floor of A is provided with a metal tray containing a piece of wet felt which fits the floor of the chamber. The entrance box may be removed by raising out of the iron straps, C and C′, the board to which it is attached. (2) B is a discrimination chamber, $26 \times 51 \times 22$. Leading from A to B is an entrance, I, 8×10. The floor of B, like that of A, is carpeted with wet felt. The tray of either compartment can be easily removed and cleaned. (3) W and W′ are electric boxes separated from each other by the partition D. On the floors of W and W′ are fitted pieces of slate, $16\frac{1}{2} \times 24\frac{3}{4} \times 1$, carrying electric wires by means of which an animal can be shocked when its behavior demands punishment. By means of the interposited sides, O O O, the electric compartments are set back 14 cm. from the stimulus shifter. The floor of this extended portion drops down about 10 cm. below that of W-W′. The middle O stands up closely to the adapter so that the exposed stimuli are set off from each

[2] Unless otherwise stated, dimensions are given in centimeters and the order of presentation is length, width, and depth—inside measurements.

other. On the end of DO is glued a piece of piano felt, P, which rubs snugly against the shifter and prevents any intermingling of the stimulus illuminations, while at the same time, the shifter can be freely moved in either direction without becoming scratched or marred by the friction. (4) N and N′ are nest boxes, $40\frac{1}{2}$ x 22 x 22. Each nest box is covered by a tightly

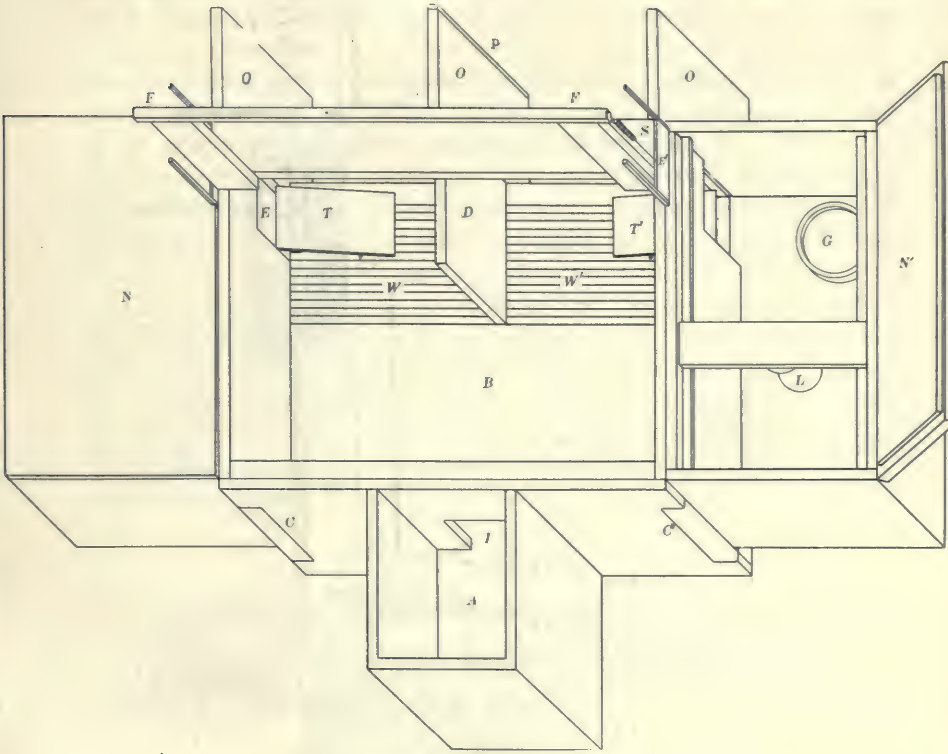


FIGURE 2. Experiment box

fitting lid, which is hinged at the outside, and is equipped with a 2 c.p. electric lamp (frosted globe), L, a watch glass for water, G, and sand or litter in which food may be scattered. It is also provided on the outer side with hidden holes for ventilation.

Between W and N, also W′ and N′, is a vertically sliding door, E closed and E′ open, 3 mm. thick which fills an opening 8 x 10. Suspended from an upright frame, F, is a coiled spring, S, which passes through the walls of N′ and B-W′ to the top

of E. When E is closed S is extended and in a state of torsion, hence when E is released S tends to return to a state of rest and the doorway is opened. The method of closing the doorway may best be seen in figure 1. A silk line, *e*, which passes up through a wire loop directly under E is attached to the

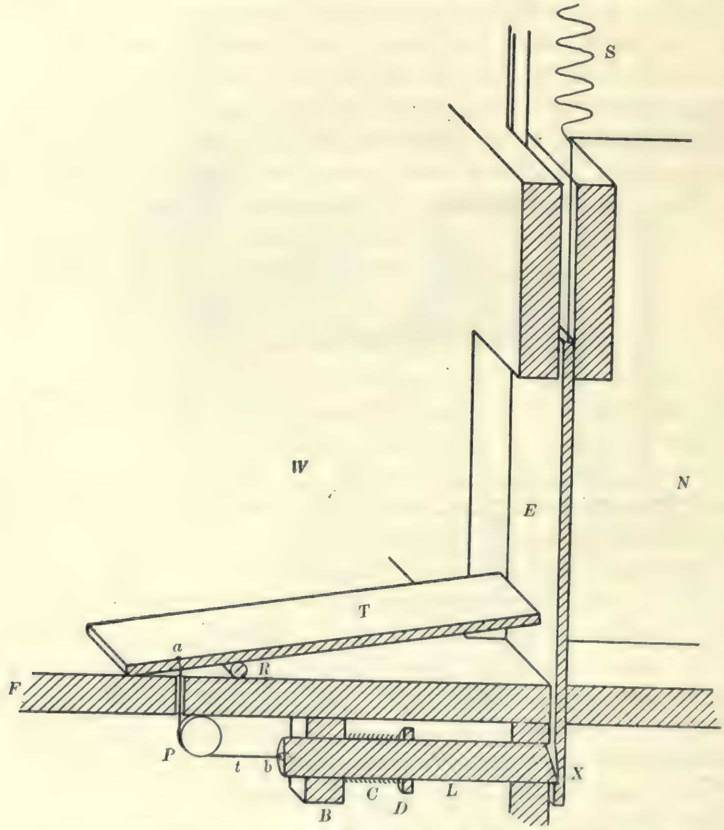


FIGURE 3. Automatic tripping device of the experiment box.

lower part of the door. By pulling on *e* the experimenter can close E which automatically locks when it is closed. By stepping on or striking T the chick can release the lock. The experimenter, however, by catching *e* on the hook *h* can prevent the door from opening when the T-lock has been released. This automatic release was devised by Professor F. S. Breed.

The construction of this mechanical device for opening E appears in figure 3 of which T is the trip that was seen in the other figures and *t* is a short piece of No. 30 black thread attached to T at *a*. Passing down through the floor, F, of the electric box, W, *t* runs over a pulley, P, and is fastened to the spring lock, L, at *b*. B is a block through which L passes and against which one end of the spring, C, presses. D is a stop attached to L supporting the other end of C. This spring tends to force L in the direction of X and to keep T in the position as illustrated. But when sufficient pressure is applied to T at any point above R, L is forced back in spite of the pressure of C, E is released at X, and the recoil of S raises E. When E is drawn down, L, by reason of the slanting end surface at X, is forced toward P until the notch of E has passed below the lower side of L when the constant pressure of C toward X forces L into the notch of E and the door is again locked.

In several respects figure 3 is a very poor representation of the tripping device. L is shown as constructed solidly in B, when, in reality, it slides very freely through B. Moreover, L is sadly disproportioned for it appears in the figure as wide as the floor of the experiment box, but it is really a delicate latch narrower than the width of T or E and is released by a very light touch upon T.

2. *Source box*

To provide illumination for the two stimuli simultaneously presented to the chick, the source box represented in figure 4, —III of figure 1,—was used. A complete description of this part of the mechanism appears in Behavior Monographs, *loc. cit.*, p. 17*ff*.

3. *Stimulus shifter*

Figure 4 also presents a general view of the stimulus shifter by means of which size, form, position, and, in conjunction with the lamp carriages, brightness of the visual stimuli can be regulated. The details of the stimulus shifter or adapter appear in the Yerkes and Watson report.

A considerable number of standard brass stimulus plates, used for varying the size and form of the visual stimulus, is required if an animal's discriminative ability is to be deter-

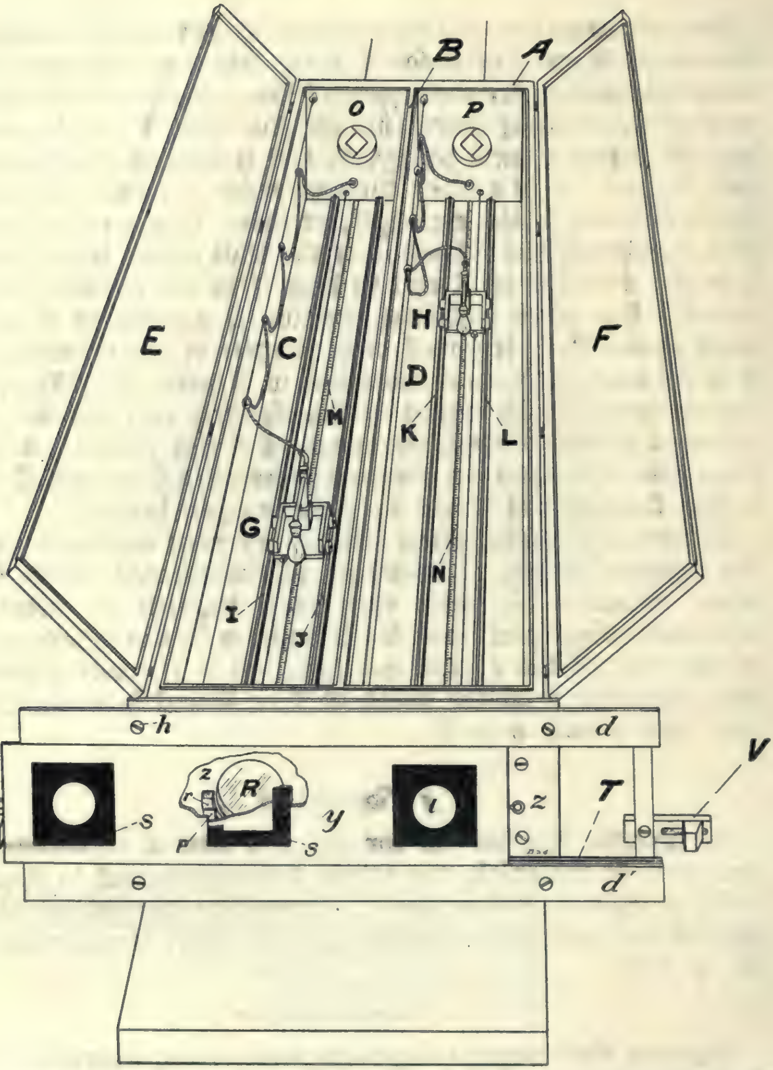


FIGURE 4. (Reprinted from *Behavior Monographs*, 1911, vol. 1, no. 2, p. 18.) "Perspective of *light* or 'brightness' apparatus. A, light box; C, D, compartments of A; B, partition between C and D; E, F, lids of A; G, H, metal carriages carrying tungsten lamps; IJ and KL, tracks for G and H; M, N, Starrett steel millimeter tapes; O, P, apertures covered by Aubert diaphragms; R, Bausch and Lomb cooling cell in light box; d, d′, metal straps; y, aluminum plate sliding between d and d′; T, tracks for y; V, stop for y; z, steel plate bolted to wooden end of light box; h, screws attaching y to z; s, s, standard brass stimulus plates; p, brass frame about aperture in y; r, hard rubber ring screwed to p."

mined. Part of the set I used, which consisted of 47 plates, is included in table 1. Owing to the variations in sensitiveness among different subjects, a set which would meet the requirements for one animal would not suffice for another, hence, in those cases where the size differences among the plates vary by one millimeter, a safe margin has been allowed by enumerating a few more plates than would ordinarily be required, and those plates for which I found no use have been omitted.

4. *Accessories*

Connected with both ends of the stimulus shifter, II, figure 1, page 67, are two ropes which pass over several pulleys and terminate at H_1 and H_2. These ropes hang 85 cm. back of the table, T, thus leaving the experimenter an abundance of free space at the front of the apparatus. They are separated from each other by 110 cm. The purpose of this attachment is to enable the experimenter to change the position of the shifter without leaving his place of observation. As is shown in the initial figure, the shifter can be moved from its present position to the right side by pulling on H_2. H_1 would thus be raised, and by pulling on it the adapter would be returned to its present position. The stops v and v' are so adjusted to the shifter that the openings in the brass plates will fall directly in the line of illumination from the source box. These stops are horizontally adjustable (not shown in the figure). The center of the pulley which v carries is $92\frac{1}{2}$ cm. above the floor. The pulleys at I, J, K, and M are 230 cm. from the floor. The distance from I to K and from J to M is 200 cm.

Arranged upon the same frame is another system of ropes and pulleys devised to control the positions of the source lamps. The terminals of these ropes are indicated by G_1 and G_2 at the experiment end, and by O_1 and O_2 at the opposite end. The drawing accurately represents the actual conditions at the experiment end, but on account of the arrangement of the walls of the dark room, O_1, O_2, R, and P merely represent the principle of the system. The ropes G_1-O_1 and G_2-O_2 are centrally attached to the source carriages of C and D. By lifting slightly upon G_1, O_1 begins to lower and draws back the lamp carriage through C. By lightly pulling down upon G_1, the carriage in C is drawn forward. The system G_2-O_2 works similarly.

TABLE 1

STIMULUS PLATES

Use	Form	Diameter or Side	Area	Number Plates Needed
To test Size		cm.	sq. cm.	
Perception	Circle	1.8	2.5447	1
"	"	1.9	2.8353	1
"	"	2.0	3.1416	1
"	"	2.1	3.4636	1
"	"	2.2	3.8013	1
"	"	2.3	4.1548	1
"	"	2.4	4.5239	1
"	"	2.5	4.9088	1
"	"	3.0 (standard)	7.0686	2
"	"	3.5	9.6211	1
"	"	4.0	12.5664	1
"	"	4.1	13.2026	1
"	"	4.2	13.8545	1
"	"	4.3	14.5220	1
"	"	4.4	15.2053	1
"	"	4.5	15.9043	1
"	"	4.6	16.6191	1
"	"	4.7	17.3495	1
"	"	4.8	18.0956	1
"	"	4.9	18.8575	1
"	"	5.0	19.6350	1
"	"	5.5	23.7583	1
"	"	6.0 (standard)	28.2744	2
"	"	6.1	29.2247	1
"	"	6.2	30.1908	1
"	"	6.3	31.1725	1
"	"	6.4	32.1700	1
"	"	6.5	33.1832	1
"	"	6.6	34.2120	1
"	"	6.7	35.2566	1
"	"	6.8	36.3169	1
"	"	6.9	37.3929	1
"	"	7.0	38.4846	1
"	"	7.1	39.5920	1
"	"	7.2	40.7151	1
"	"	7.3	41.8540	1
"	"	7.4	43.0085	1
"	"	7.5	44.1787	1
"	"	8.0	50.2656	1
"	"	8.5	56.7455	1
"	"	9.0 (standard)	63.6174	2
				44
To test Form		cm.	sq. cm.	
Perception	Circle	6.0 (as above)	28.2744	–
"	"	5.9	27.3397	1
"	"	5.8	26.4208	1
"	"	5.7	25.5176	1
"	"	5.6 and smaller circles used in size perception	24.6301	1
"	Square	5.317 (side)	28.2743	2
"	Equilateral △	8.081 (side)	28.2743	2
	Openings inscribed in circle—28.2744			
"	Square	4.243 (side)	18.003	2
"	Equilateral △	5.196 (side)	11.691	2
				12
				Total 56

The experimenter is thus enabled to control the intensities of the stimulus areas without leaving his end of the apparatus. By moving a carriage backward the intensity of the corresponding stimulus display is decreased; by drawing the source nearer, the intensity is increased.

Figure 1 also shows arranged on a table, T, the electrical connections with the experiment box. X, Y, and Z are respectively an inductorium, a Columbia dry cell (No. 6), and a telegraph key which, in connection with the wired slate floors of W and W′, constitute an interrupted circuit. The wires are wrapped alternately about the slate floors so that by closing the key, Z, a chick, when its feet touch any two of the wires, can be shocked. With young chicks under two weeks of age it was found necessary in general to set the secondary coil at 5, but as they become older and heavier the strength of the shock had to be decreased, until, in some cases, the position of the secondary coil was as far out as 6.5. The letters *s* and *s′* indicate electric switches by means of which the illumination in N and N′ can be turned on or off. An upper illumination, L, designed to hide inequalities in the distribution of light in each compartment, W and W′, hangs about 120 cm. above the floor and directly over the center of the electric boxes. It consists of a 2 c.p. electric lamp inclosed in a vertically suspended cylinder of galvanized iron. The diameter of this cylinder is 10 cm. and its length is 20 cm. The upper end is closed. The lower end is fitted with a cover (like that of a baking powder can) which may be pushed on or off. Centered in this cover is an Aubert diaphragm by means of which the amount of light falling on the experiment box may be regulated. On account of the low ceiling of the dark room where these experiments were conducted, I found it impracticable to use the diaphragm. Unless it can be raised to a sufficient height it will cast shadows upon the floor of the experiment box. With the diaphragm removed, the upper illumination consisted of the rays from a 2 c.p. lamp passing through a circular opening 10 cm. in diameter and falling a distance of 120 cm.

The importance of L will be understood more thoroughly in connection with the conditions for size discrimination. If a chick is being trained to choose ⊙ 28+,[3] (a circle whose area

[3] The dimensions of stimulus areas refer in square centimeters to the area.

is 28.2744 sq. cm.), and to reject ⊙ 7+, unequal amounts of light from the stimulus sources will be admitted to the electric compartments. If the intensities of the two stimulus areas be equalized, the compartment at the end of which the larger circle appears will be the more highly illuminated. Without the elimination of this factor of general illumination, the experimenter can not be certain that his animals are discriminating on the basis of size; they may be choosing the *lighter* compartment. To overcome this difficulty the upper illumination was used and the elongation of the electric compartments, O O O, figure 2, was introduced. The amount of upper illumination should therefore depend upon the degree of difference between the two stimulus areas, and in a quantitative study, where the difference is continually being reduced toward the minimum of the animal's discriminative ability, the difficulty practically disappears.

A further value of the upper illumination is economy of time for the experimenter. It aids in emphasizing, from the first, the visual factor which is being tested and thus prevents the animal from acquiring a habit of discrimination which later must be inhibited and replaced by a different habit. After an animal once acquires a definite way of reacting to stimuli, no little time and pains are required to make it change its mode of response.

With the elongation of the electric compartments appears another difficulty differing somewhat from the matter of general illumination. This is the problem of the chick's optimal focal distance. It is quite possible that, for a stationary stimulus, the discrimination chamber of this apparatus is too far away. The anatomy of the eye as well as the behavior of the animal should be considered in connection with this problem. The matter of distance perception, however, falls without the scope of the present task.

II. PROBLEM, METHOD, AND TECHNIQUE

1. The chicks

The experiments reported in this paper were made with about 25 chicks belonging to three different groups. The first group, originally consisting of 10 chicks, was secured from a

poultry breeder when the chicks were about two days old. The second and third groups were artificially incubated in the laboratory. All of the chicks used were of the Barred Plymouth Rock variety. The matter of caring for the birds and keeping them healthy was one of the most serious difficulties which I had to overcome. On the whole, I found it more satisfactory to use the laboratory hatched chicks.

The most common ills were bowel trouble and "leg weakness." Both types of disease were chiefly due to improper feeding, temperature, and ventilation. My experience leads me to conclude that chicks in close confinement must be fed on "starving rations." A few individuals of group 3 survived until the weather became warm enough to get them out of doors during a few hours on favorable days. When this plan was first tried the birds were in poor condition. Two-thirds of the group had already died. But as soon as the survivors were placed out of doors their physical condition began to improve, and I was able to do nearly three times the amount of experimental work that I could formerly accomplish. Apparently the factors that healthy laboratory chicks require is an abundance of sunlight and fresh air with an opportunity to work for their living.

The "leg weakness" is a type of disease which had previously given trouble in the laboratory. The leg joints become enlarged, the toes curl out of shape, the birds cannot stand, and they move about only with great difficulty. This trouble ultimately carried off nearly all of the birds which did not succumb to bowel trouble. In the case of one brood I think it was the result of excessive heat in the brooder. In other cases it was probably due to overfeeding. More often, perhaps, it was due to a combination of both conditions. The birds which first showed signs of this weakness were the largest and apparently strongest of the flock. There was no evidence of it among the chicks of the third group, which were fed very sparingly, while the temperature of the brooder was carefully regulated.

2. *The problem*

The matter of health among the chicks turned out to be a problem which had not been anticipated. However, it did not prevent work toward the solution of the primary problem. The

task originally planned was a study of the chick's discriminative ability between sizes, forms, and brightnesses, but, owing chiefly to these unfavorable conditions, little consideration has been given to the third factor. The present paper is a preliminary report of a detailed investigation which the writer hopes to complete within a few years.

As far as the study goes, the aim has been to make it intensive and quantitative. In the matter of size and form, the question has been, not merely: Can the chick discriminate between stimuli which differ from each other with respect to one of these factors, but What is the least difference that it can discriminate? Breed's work [4] has indicated that chicks *can* discriminate on these bases. The work of Katz and Révész [5] suggests the same possibility. My original plan, therefore, was to test this matter quantitatively and to determine the chick's threshold of difference for each of these factors.

With respect to size, I have carried out my original plan without any essential changes, but in the case of form, I was forced to abandon the plan with which I started. In a short time it appeared that proper responses to stimuli differing from each other only with respect to form were not so readily acquired as reactions to size differences. After I had tried in vain for several weeks to train different subjects to discriminate between a circle and a triangle which were equal in area, the nature of my problem was markedly changed. It was clearly necessary to determine whether the chick, under the conditions of this experiment, could perceive form.

Finally, another aspect of the problem which quite naturally appears in an investigation of this sort is that of the relative value of visual size, form and brightness. My earlier results with forms, negative beyond all doubt, emphasized the desirability of considering the factors in combination as well as in isolation. In its normal life the chick is not compelled to rely upon a single visual factor, but, on the contrary, it relies upon a natural combination of visual qualities. It thus seemed important to start with complex stimuli and from these grad-

[4] Breed, Frederick S. The development of certain instincts and habits in chicks. *Behavior Monographs*, 1911, vol. 1, no. 1, S. N. 1; also Reactions of chicks to optical stimuli. *Jour. Animal Behavior*, 1912, vol. 2, pp. 280-295.

[5] Katz, D. and Révész, G. Experimentell-psychologische Untersuchungen mit Huhnern. *Zeit. f. Psych. u. Physiol. d. Sinnesorgane*, 1909, Bd. 50, S. 93.

ually to eliminate the inequalities which were not wanted, until only the visual factor in question remained.

3. Method and technique

These changes in the problem are closely allied with the mode of procedure. The mechanical phase of my method appears in the description of the apparatus, but it does not fully explain the application of this procedure to the study of the chick's visual perception. This topic might well be considered in two parts:—the one dealing with the mechanism, the *objective aspect*, and the other relating to the plan of procedure, the *personal aspect* or *technique*. In this report I shall not attempt to make such a sharp distinction, for while the two topics are, in many respects, clearly distinct, I find them so closely interrelated that it is impracticable to consider them separately. In general, however, I shall devote the first part of this section primarily to method, and in the latter part, the discussion will be continued more from the standpoint of technique.

The plan of the apparatus in relation to the chick is to provide conditions both desirable and undesirable. Either nest box includes those factors which the chick wants. It provides food, light, warmth and companionship. The experiment box is arranged to make the chick want to get out. In the entrance box the chick is closely confined; in both the entrance and discrimination chambers the floors are wet; the entire experiment box, exclusive of the nest boxes, provides faint illumination, little warmth, no food and no companionship. When a chick, familiar with the nest boxes, is placed in the entrance box its natural desire is to get back to one of the nest boxes. Its problem, then, is to learn how to get to the desirable part of the apparatus.

The two visual stimuli which are displayed on the stimulus shifter indicate to the animal which nest box to choose. During the training series, both electric compartments are identical except for the difference between the two stimulus areas. Since the animal wants to reach the nest box it will try to find a way of accomplishing this end.

Each chick was taught the way of escape to the nest box by means of 20 preliminary trials. The entrance to the nest

box on that side where the *right* stimulus appeared was open; the sliding door closed the entrance from the electric box where the *wrong* stimulus was presented. The chick was allowed to go now to one, now to the other nest box, in irregular sequence until it had found its way 10 times to each. It was thus made familiar with nest boxes and the experiment box.

By displaying, always on the side of escape, the stimulus which the chick was later to be trained to choose, the animal was occasionally aided in acquiring a perfect habit.[6] This condition was especially noticeable in the experiments on size discrimination. In this case a circle 6 cm. in diameter always appeared in that electric compartment from which the subject escaped. At the end of the preliminary series a few chicks had acquired a perfect response to this condition of ⊙ 28+—⊙ 7+ discrimination.[7] This perfect ⊙ 28+—⊙ 7+ habit, however, may not mean that size was the basis of choice. It is quite possible that the birds chose the lighter compartment. Precaution was always taken to eliminate this possibility before size tests were completed, and control tests were carefully planned to make certain that it *had* been eliminated.

This preliminary work was followed by the training series. Both entrances to the nest boxes were now closed, and the only cues that remained to aid the chick to reach the nest box were the two optical stimuli. Continuing the example of size discrimination, ⊙ 28+ was the positive sign, ⊙ 7+ was the negative sign of escape from the experiment box. At this point in the training the electric shock was introduced. If a chick chose ⊙ 7+, *i.e.*, stepped upon the wires of that compartment, it was shocked. This was done by momentarily closing the key, Z, (figure 1). A single shock was usually given, but if the behavior of the bird indicated that the shock had not been sensed, it was repeated. The wet floors of A and B served to regulate the intensity of the shock. Great care was necessary to provide a shock that was an effective punishment yet not severe enough to frighten the animal. Care in the manipulation of the shock was also essential. The results of weeks of work could be destroyed in a moment through an error in the administration of punishment.

[6] A habit is termed perfect when a chick successively makes 20 correct choices.

[7] The stimulus demanding a positive response is named first; following this is the stimulus demanding a negative reaction.

It was quite common for chicks that were being trained to go beyond the door to the nest box and crowd up to the stimulus at the end of the compartment. If the animal were allowed to do this it spent considerable time and energy trying to get through the illuminated stimulus plate. A thin plate of glass was placed across each compartment at a point just beyond the exit. The chicks could thus see through the stop but could not go beyond it. They thus were stopped near the exit so that they might learn more readily how to escape. As soon as they had acquired a mode of escape and had ceased trying to reach the stimulus, the glass plates were removed.

After a perfect habit had been acquired the amount of difference between the two stimuli was decreased. When the ⊙ 28+—⊙ 7+ discrimination, for example, was perfect, the condition was changed to ⊙ 28+—⊙ 9+. The large circle was thus the standard which remained constant. The smaller circle was the variable, which, after each perfect reaction, was successively increased in size until the animal could no longer choose correctly. This limit of correct choices was thus accepted as the chick's threshold of difference.

Table 1, page 74, provides for a series of stimulus plates that will enable the experimenter to make the most satisfactory changes in the variable stimulus. The changes in diameter can well be as great as 5 mm. between ⊙ 7+—and ⊙ 12+ when used with a standard ⊙ 28+. Above ⊙ 12+ the diameter of the variable should be successively increased only by 1 mm. The set of plates with which I started included a group between ⊙ 19+ and ⊙ 28+ varying in diameter by 1 mm., but provided only a ⊙ 16 between ⊙ 12+ and ⊙ 19+. A similar scale was provided between ⊙ 50+ and ⊙ 63+ but there were no variables between ⊙ 28+ and ⊙ 50+. After a little preliminary work, however, I discovered that a different scale of variables was necessary, and in table 1 I present the sizes which my experience leads me to regard as essential. Unless he wishes to make a study of that particular problem, the experimenter, in using variables differing in diameter by only 1 mm., must be alert to see that the natural discrimination of his subjects is not improved by training.

During the experiments with the first group of chicks, I was seeking to get a method as much as to train the birds. I can

RECORD SHEET.

Subject 9 **Date** 12/7/'21–1/2/'22 **Experiment** Form Perception

Tests → / Series ↓	1	2	3	4	5	6	7	8	9	10	R	W	REMARKS Average Time
A	l	r	l	r	l	r	l	r	l	r			
B	r	l	r	l	r	l	r	l	r	l			
1	r E-29	l 0-6	r E-59	l 0-19	r E-125	l 0-120	r E-56	l E-95	r E-65	l 0-44	4	6	61.8"
2	l 0-4	l 0-24	r 0-36	r 0-30	l E-1-62	r 0-39	l 0-19	l E-2-180	r 0-15	r 0-33	8	2	44.2
3	r 0-67	r 0-8	l E-135	r 0-28	l 0-25	l E-2-141	r E-1-180	l E-1-21	r 0-5	l E-1-61	5	5	67.1
4	l 0-29	l E-1-42	l E-1-35	r 0-55	r 0-40	r 0-64	l E-2-100	r 0-30	r 0-32	l E-1-65	6	4	49.2
5	r E-2-84	l 0-8	r 0-9	l 0-12	r 0-10	l 0 6	r 0-18	l 0-26	r E-1-43	l E-2-38	7	3	25.4
6	l 0-11	l 0-14	r E-2-71	l 0-21	r 0-83	r E-2-41	l 0-28	r E-2-110	l 0-49	r E-2-118	6	4	54.6
7	r 0-48	l 0-31	l E-2 70	l 0-7	r E-1-8	r E-1-23	r 0-25	l 0-6	r 0-9	l E-1-56	6	4	28.3
8	r E-2-46	r 0-62	l 0-89	l 0-67	r E-2 71	l 0-58	r E-1-65	l E-1-56	r 0-24	l E-1-29	4	6	56.0
9	r 0-4	r 0-5	r 0-6	l E-1-20	l E-1-7	l 0-21	r E-2-42	l E-1-23	r 0-15	l E-1-16	5	5	15.9
10	l 0-4	l 0-9	l 0-5	l 0-31	r 0-106	r 0-19	r 0-61	r 0-5	l 0-10	r 0-45	10	0	29.5
11	r 0-10	l E-2-124	r E-2-128	r 0-35	r E-2-75	l 0-67	l 0-34	l 0-26	r 0-38	l E-1-88	6	4	60.2
12	r 0-6	l 0-37	r 0 22	l 0-10	r 0-14	r 0-12	l 0-8	l 0-8	r E-1-36	l E-1-14	8	2	16.7
13	r 0-8	l 0-7	r 0-40	l 0-45	l E-1-110	l 0 3	r 0-1-29	r E-1-20	r 0-21	l E-2-44	7	3	32.7
14	l 0-12	l E-1-123	l E-1-31	l 0-10	r E-1-10	r 0-74	r 0-12	r E-1-27	l 0-8	r E-1-31	5	5	33.8
15	r 0-38	l 0-11	r 0-22	r 0-8	r E-1-38	l E-2-54	l E-2-64	l 0-16	r E 1-39	l 0-5	6	4	29.5
16	l 0-18	r 0-8	l 0 21	l 0-30	l 0-5	r E-1-46	r E-1-42	r 0-33	l 0-11	r 0-28	8	2	24.2
17	r 0-48	r 0-37	r 0-40	r 0-11	l E-1-19	l E-1-50	l 0-62	l 0-30	r 0-28	l E-1-19	7	3	42.8
18	l E-1-60	r E-1-94	l 0-25	r E-1 35	r 0-20	l E-1-24	l E-1-25	r 0-15	l E-1-18	r 0-12	4	6	32.8
19	r 0-92	l 0-40	r E-1-46	l E-1-76	r 0-25	l E-1-58	r E 2-45	l 0-10	r E 2-27	l E-1-33	4	6	44.7
20	l E-1-12	l 0-39	l 0-8	r E-2-17	l 0-18	r 0-25	l 0-9	r E-1-60	r 0-42	r E-1-103	6	4	33.3
21	r E-1-22	l 0-12	l 0-13	r E-1-15	r 0-17	l E-1-26	l 0-11	r E-2-10	r 0-19	l E 1-40	5	5	18.5
22	l E-1-65	l 0-19	r E-1-40	r E-1-20	l 0-7	l 0-21	r E-1-22	r E-2-30	l E-1-46	r E-1-25	3	7	29.3
23	r 0-12	l 0-25	l 0-10	l 0-8	l 0-4	r E-2-41	r E-1-9	r 0-2	r E-1-10	l E-1-12	6	4	13.3
24	l 0-5	r E-1-49	l E-1-6	l 0-3	l 0-5	r 0-4	r 0-4	r 0-4	l 0-2	r E-1-45	7	3	12.7
25	r 0-14	r 0-12	r E-1-6	r 0-26	l 0-25	l 0-23	l 0-18	l 0-15	r 0-55	l 0-25	9	1	27.8

not here attempt to show how the method was developed. Only its final form can be presented. A form of *record sheet* had previously been used in the laboratory for tabulating experimental results in the discrimination method. This blank was well suited to my needs. It provides spaces at the top for recording the name or number of the subject, the date, and the experiment. Across the sheet are 10 vertical columns in which may be entered the result of each test. Each group of 10 tests constitutes a series. The sheet provides for records of 25 series with a space for two (A and B) preliminary or preference series. The result of 250 tests, therefore, may be put on one record sheet. The vertical columns headed *R* and *W* are for the total number of *right* and *wrong* choices made by the subject during a series of 10 tests. Another vertical column leaves space for remarks.

This record sheet does very well for keeping a concise summary of the experiments, but it does not enable one to keep

TEST SHEET

Title of investigation, Form: ⊙ 28+—△ 28+
Experimented on, 9
Harvard Psychological Laboratory, December 25, 1912
Record Sheet, 1; Series, 15

Test	Behavior	Record
1 r	$/\overline{/}$ – /// + /// *u*	O–38
2 l	/// + /// *u*	O–11
3 r	$\overline{//}$ + /// *u*	O–22
4 r	// + // *u*	O–8
5 r	/// – // $\triangle^1$ + /// *u*	E–1–38
6 l	/// – /// $\triangle^2$ + // *u*	E–2–54
7 l	// – /// $\triangle^2$ + /// *u*	E–2–64
8 l	// + // *u*	O–16
9 r	// + / $\triangle^1$ + // *u*	E–1–39
10 l	/ + // *u*	O–5

6–4–29.5

REMARKS:—Tendency to choose by position,—*i.e.*, to go where it last escaped. Usually goes directly to other compartment when it has been shocked for wrong choice.

the details of the behavior. To do this I had a form of *test sheet* printed. It is headed by spaces for the title of investigation, the name of the subject, and the date; also there are spaces for reference to the *record sheet* and the particular series of the *record sheet* which the *test sheet* contains. The 10 tests, (horizontally arranged on the record page), appear vertically on the test page. After each one is a space for recording the details of the animal's behavior. At the right of the page is a narrow column for recording the results of the test,—whether or not an error was made, and the time for choosing. At the bottom is a space for remarks, where the record is always totalled and averaged,—total number of right and wrong choices and average of time in seconds. The record and this part of the remarks are transferred to their proper spaces in the record sheet.

The manner of recording the chick's behavior is illustrated in the accompanying *test sheet* which is a record of an actual series of tests. I used a set of symbols which enabled me to follow fairly accurately the movements of an animal while in the discrimination chamber. Herewith is presented the key to the symbols used in these experiments:

1. $+$ = Approach to *right* compartment.
2. $-$ = Approach to *wrong* compartment.
3. $/$, $//$, $///$ = Degree of attention based on behavior and time. A horizontal bar above any one of these symbols indicates unusually long time in this locality. For example,
   - a. $+/$ = Brief consideration of *right* stimulus area.
$-/$ = Brief consideration of *wrong* stimulus area.
   - b. $+//$ = Longer consideration of *right* stimulus area.
$-//$ = Longer consideration of *wrong* stimulus area.
   - c. $+///$ = Close consideration of *right* stimulus area.
$-///$ = Close consideration of *wrong* stimulus area.
   - d. $+\overline{///}$ = Long time before and close consideration of *right* stimulus area.
$-\overline{///}$ = Long time before and close consideration of *wrong* stimulus area.
   - e. $+\bar{/}$ = Long time before but slight consideration of *right* stimulus area.
$-\bar{/}$ = Long time before but slight consideration of *wrong* stimulus area.
4. $\overline{//////////////}$ = Attention at entrance end; indifferent to stimulus areas.
5. $\wedge$ = Approach on wires before *right* stimulus followed by retreat.
6. $\vee$ = Approach on wires before *right* stimulus followed by wrong turn.
7. $\triangle$ = Approach on wires before *wrong* stimulus followed by retreat.
   - a. $\triangle^1$ = One shock.
   - b. $\triangle^2$ = Multiple shock.
   - c. $\triangle^0$ = No shock.
8. $\bigcirc$ = Turn around, right to left; $\breve{\bigcirc}$, left to right.

9. ∾ = Partial turn around, right to left; ∽, left to right.
10.)(= Position directly before division between each compartment; by turning head either stimulus area is visible.
11. ⁄ = Escape to nest box.
12. E = Error in choosing; this is followed by time in seconds.
13. O = Correct choice; this is followed by time in seconds.

The preliminary series were begun during the second week, usually when the chicks were 10 days old. The experiments were always conducted during the forenoon between the hours of eight and twelve. I believe the best results would be obtained from chicks if the work were begun earlier than eight. Chickens naturally start out early in the morning for their food. That is the time when they are most active. My observations do not wholly agree with Breed's conclusion that "hunger did not play a more important part than the reaction to confinement and solitude."[8] My best results came when I took the chick early in the morning and allowed it to earn its breakfast. The chick from which I received the best results reached the stage where it apparently took pleasure in the experiments. When I placed it in the apparatus it regularly began to give the characteristic "food twitter." This twittering was continued all of the time the bird was choosing and after it had escaped to the nest box where I was careful always to have a little food.

In the matter of rewarding with food, however, great care is necessary. A three-weeks-old chick will "fill up" in a very short time and then experimental work is difficult. I was careful to have only a few grains scattered in the litter of the nest boxes so that the chick had to work to find them. After getting this plan somewhat perfected I found that a chick could be given as high as 50 trials with no more difficulty than was previously common in 20 trials.

Until the chicks were five or six weeks old it was found best to give them no more than one series daily. After they reached this age the number of tests could be increased without harmful results. The subjects had usually learned to solve their problems by this time so that they expended much less energy than they did during the earlier tests. Moreover, the experimenter had an opportunity to select from his group the most promising subjects to which he could devote more time.

---

[8] *Behavior Monographs*, vol. 1, no. 1, S. N. 1, p. 47.

4. *Importance of method*

In the preceding section I have discussed in considerable detail the method and technique of my experiments. I do this because I believe an accurate solution of an animal problem depends upon the adoption of a favorable method. It is one of the greatest tasks that confronts the experimenter. He must use an apparatus by means of which he can accurately control the conditions of his study. He must also show alertness by controlling these conditions in such a way that his animals behave normally. Much animal stupidity, so-called, is really a reflection of human ignorance. Experimenters frequently lack animal intelligence by setting human problems for an animal to solve.

An animal frequently shows ingenuity in an unexpected direction, and if the experimenter be not alert he misses the most important part of the behavior. How many times this is the case, we cannot tell, for we only know of the cases where we did not miss the significant fact. A perfect method would make it impossible for the animal to pick up cues that were not intentionally offered by the experimenter. But it is through experience that we approach a perfect method for there is always the possibility of finding defects even in the best methods.

The importance of method was impressed upon me very forcibly through an incident in my experiments with the first group of chicks. I had inexcusably blundered by making the original conditions of discrimination too difficult for the birds. They were being tested on ⊙ 28+—⊙ 7+ discrimination. The subjects were allowed to go through the discrimination chamber and experiment box during the preliminary tests without regard to the position of the right or wrong stimulus; that is, both exits were open. When the training series were begun the brightness of the two stimuli were made markedly unequal and were irregularly varied *from the first.*

The experiment was begun with six subjects. As a partial result of my initial bungling, I had, at the end of a few days, only subjects 2 and 3 in suitable condition for experimental work. At the end of two and one-half weeks No. 2 gave up, but No. 3 persisted. At the end of the 24th series it had successively made three perfect series (see table 2). At the end of four more series it had reacted perfectly to the ⊙ 28+—⊙ 9+

discrimination. The variable was continually increased by 5 mm. (diameter) until the condition ⊙ 28+—⊙ 19+ was reached

TABLE 2

Subject: 3. Hatched: 10/3?/'11. Sex: Undetermined

Series*		Date	Right	Wrong	Time†
		⊙ 28+—⊙ 7+ *Discrimination*			
1–21	(1–21)	Oct. 12–27	..	..	..
22	(22)	" 28	10	0	55
23	(23)	" 30	10	0	41
24	(24)	" 30	10	0	29
		⊙ 28+—⊙ 9+ *Discrimination*			
1	(1)	Oct. 30	10	0	62
2	(2)	" 31	9	1	79
3	(3)	" 31	10	0	103
4	(4)	Nov. 1	10	0	55
		⊙ 28+—⊙ 12+ *Discrimination*			
1	(6)	Nov. 2	7	3	71
2	(7)	" 3	9	1	93
3	(8)	" 3	10	0	50
4	(9)	" 4	10	0	52
		⊙ 28+—⊙ 15+ *Discrimination*			
1	(11)	Nov. 4	6	4	112
2	(12)	" 6	9	1	68
3	(13)	" 6	8	2	44
4	(14)	" 6	8	2	51
5	(15)	" 7	9	1	52
6	(16)	" 7	9	1	40
7	(17)	" 7	8	2	41
8	(18)	" 8	10	0	52
9	(19)	" 8	10	0	62
		⊙ 28+—⊙ 19+ *Discrimination*			
1	(21)	Nov. 8	8	2	49
2	(22)	" 9	8	2	49
3	(23)	" 9	8	2	90
4	(24)	" 9	10	0	50
5	(25)	" 10	8	2	78
6	(26)	" 10	10	0	53
7	(27)	" 11	10	0	47
		⊙ 28+—⊙ 23+ *Discrimination*			
1	(6)	Nov. 11	8	2	58
2	(7)	" 13	8	2	116
3	(8)	" 14	10	0	41
4	(9)	" 15	8	2	45
5	(10)	" 15	10	0	16

* Numbers in parentheses refer to series on record sheet.
† Average time for series given in seconds.

TABLE 2—*Continued*

SUBJECT: 3. HATCHED: 10/3?/'11. SEX: UNDETERMINED

Series*		Date	Right	Wrong	Time†
		⊙ 28+—⊙ 24+ *Discrimination*			
1	(21)	Nov. 17	10	0	22
2	(22)	" 17	9	1	30
3	(23)	" 18	10	0	16
4	(24)	" 18	10	0	11
		⊙ 28+—⊙ 25+ *Discrimination*			
1	(11)	Nov. 21	9	1	22
2	(12)	" 21	10	0	16
3	(13)	" 21	9	1	17
		⊙ 28+—⊙ 26+ *Discrimination*			
1	(16)	Nov. 22	10	0	15
2	(17)	" 22	9	1	24
3	(18)	" 23	7	3	45
4	(19)	" 23	10	0	23
		⊙ 28+—⊙ 27+ *Discrimination*			
1	(21)	Nov. 23	8	2	20
2	(22)	" 23	8	2	25
3	(23)	" 24	6	4	33
4	(24)	" 24	10	0	11
5	(25)	" 24	9	1	30
		⊙ 28+—⊙ 28+ *Discrimination*			
1	(1)	Nov. 25	10	0	?
2	(2)	" 25	6 (crack closed)	4	?
3	(3)	" 25	0	5	?
		⊙ 28+—⊙ 23+ *Discrimination*			
1	(16)	Nov. 27	5	5	62
2	(17)	" 27	5	5	59
		⊙ 28+—⊙ 19+ *Discrimination*			
1	(18)	Nov. 28	3	7	71
		⊙ 28+—⊙ 15+ *Discrimination*			
1	(19)	Nov. 28	9	1	60
		⊙ 28+—⊙ 19+ *Discrimination*			
1	(20)	Nov. 29	6	4	73
2	(21)	" 29	5	5	83
3	(22)	" 30	7	3	31
4	(23)	" 30	7	3	22
5	(24)	" 30	5	5	23
6	(25)	Dec. 1	5	5	27

* Numbers in parentheses refer to series on record sheet.
† Average time for series given in seconds.

after which the diameter of the variable was changed by increments of 1 mm. The perfect responses continued almost as regularly as the variable was increased. Finally the behavior of the chick indicated that it was discriminating between ⊙ 28+ and ⊙ 27+ (see record of November 23 and 24).

A ⊙ 28+—⊙ 27+ discrimination was incredible, for the human eye could scarcely detect a difference between a 28+ sq. cm. circle and a 23+ sq. cm. circle. The only thing to do was to test the chick on ⊙ 28+—⊙ 28+ discrimination. The result was a perfect series. (See record 1, November 25.) Then I began to look for the cue on which the chick relied. At last I noticed a small crack where the outside extension of the electric box was joined to the experiment box. A similar crack appeared on both sides in corresponding positions, hence it seemed that in this factor there could be no clue by means of which the chick was guided in its choice of the *right* compartment.

Close inspection, however, proved that in these two minute cracks lay the cue by means of which the chick had been discriminating. Where the rabbeted edges of the shifter and tracks rubbed, there was a bright edge, and wherever the shifter rested, this bright surface was covered. Thus when the shifter was at the left, the right end of the track was uncovered, and from this uncovered part was reflected some of the light rays from the upper illumination. A small portion of this reflected light could enter the crack on the left side. Now, when the standard circle was presented at the left, it happened that the shifter was moved to the right; when the standard appeared at the right, the shifter stood at the left. As a result, the crack that was illuminated was always the one where the standard circle was displayed; the other crack, being reached by no reflected light, was always dark.

Noticing this *slight* variation in the condition of illumination in the two compartments, and desiring to ascertain by what clue the chick was choosing, I closed the small cracks through which the light was reflected. The effect of this change appears in the records following the first on November 25. The perfect reactions abruptly stopped with the closing of the cracks; out of 15 tests there were only six correct responses. As appears in the subsequent part of table 2, I continued the experiment, *with the cracks stopped*, by reducing the area of the variable to

23+, (November 27), then to 19+ and finally to 15+ before the chick again began to react correctly to the stimuli. When the ⊙ 28+—⊙ 19+ discrimination was again presented, the subject was able to choose correctly in little more than half of the tests. The physical condition of No. 3 on December 1 made it necessary to abandon further work. Up to that date it had appeared in all respects normal, but the final series was conducted with great difficulty on account of the common "leg-weakness" which had suddenly developed. With the appearance of this disease, the experiment was abruptly closed.

The chick, then, had detected a faint streak of light, not more than 6 mm. long and less than 1 mm. wide,—an illumination so small that not only I but others who were working in the laboratory had quite overlooked it. By considering the amount of light that came from the upper illumination, the poor reflecting surface of a steel strap worn only moderately bright, and the narrow surface from which the light was reflected, (not more than 1 cm. in width), the reader can judge how insignificant this strip of light would appear. It was clearly a case of the chick "outguessing" the experimenter.

III. SIZE PERCEPTION

1. Discrimination between unequal circles

The foregoing discussion of method suggests the nature of the difficulties which were encountered in the early experimental work. No trustworthy results were secured until after the crack of light was discovered. By this time No. 3 was the only subject of the first group that was still working. The later experiments with this chick indicated that the early discrimination had been made on the basis of size difference, but as the variable became larger and the discrimination correspondingly harder, No. 3 began to look for other cues and happened upon the one which has been described.

A glance at table 2 suggests the point where No. 3 ceased to discriminate between the two size stimuli. After the control tests, ⊙ 28+—⊙ 28+ discrimination, during which the crack was stopped, the chick's response to ⊙ 28+—⊙ 23+ was tested. The results were decidedly negative since the *right* stimulus was chosen no more than chance would allow. The response to

⊙ 28+—⊙ 19+, having only three right choices, was even more negative, but when the ⊙ 28+—⊙ 15+ discrimination was tested, the results were clearly positive. The standard was chosen nine times and the variable was chosen only once. Here is probably the place where, in the earlier training, the new cue first figured. While the average results of the tests with the ⊙ 19+ variable are slightly in favor of the larger stimulus, I believe the fact should not be emphasized, for the larger circle was chosen in 60 trials only five more times than would have occurred had chance alone determined the choices.

Table 3 summarizes the results of my investigation on the chick's perception of size. In some cases the results are not so clear cut as one might wish, but this fact is due to the uncertainty of health among the birds which made it advisable to hurry the tests, without waiting in some cases for the acquisition of perfect habits, in order to take the chicks to their limit in discrimination.

By considering not only the right-wrong records but also the peculiarities in behavior which cannot be presented in tabular form, I am convinced that the largest variable the chick can distinguish, under the conditions here described, from a standard ⊙ 28+ lies somewhat above ⊙ 15+. The quantitative measurements on the basis of right choices points strongly to this conclusion. Every subject, excepting 7 and 20, succeeded in making at least 88% of correct choices in a series when the ⊙ 28+—⊙ 15+ discrimination was required. Even in the case of the two exceptions, 7 and 20, an efficiency of 70% was acquired. But more convincing still is the fact that two chicks, 3 and 17, reached 90%, and two, 16 and 21, made perfect series.

The time used in choosing is a factor which offers a means of measuring quantitatively the chick's threshold of discrimination. It should not be unduly emphasized for it is not wholly reliable, yet in these experiments it tends to substantiate the conclusion which has been based upon the percentage of right choices. Since the animal is less acquainted with the conditions during the ⊙ 28+—⊙ 7+ discrimination as compared with the ⊙ 28+—⊙ 15+ discrimination, it would be expected, other things remaining constant, to choose more readily during the later tests. Furthermore, the increase in age and activity should enable it to choose more rapidly during the later tests.

TABLE 3

Discrimination Between Unequal Circles

Series	No. 3[1]						No. 6♂						No. 7♂						No. 15♂[2]						No. 16♂[2]					
	28+ 7+		28+ 12+		28+ 15+		28+ 7+		28+ 12+		28+ 15+		28+ 7+		28+ 12+		28+ 15+		28+ 7+		28+ 12+		28+ 15+		28+ 7+		28+ 12+		28+ 15+	
	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W
1.....			5	5	9	1	7	3	8	2	8	2	8	2	10	0	7	3	9	1	10	0	7	3	9	1	8	2	10	0
2.....			8	2	[3]		9	1	8	2	[4]		6	4	9	1	7	3					7	3						
3.....			6	4			9	1	8	2			7	3	7	3	[4]						8	2						
4.....			8	2			8	2	6	4			9	1	8	2														
5.....			9	1			10	0	8	2			9	1	9	1														
6.....			6	4			9	1	6	4			9	1	8	2														
7.....			10	0			8	2	8	2			10	0																
8.....							6	4					8	2																
9.....							9	1					10	0																
10.....							10	0					10	0																
11.....							8	2					10	0																
12.....							10	0					10	0																
13.....							10	0					10	0																

[1] Preceded by tests which were ruled out on account of the crack of light.

[2] The record for $\odot$ 28+ — $\odot$ 7+ was preceded by 13 series in which the light factors were combined; the record given represents the first series after all factors except size were eliminated.

[3] Followed by $\odot$ 28+ — $\odot$ 19+ in which No. 3 failed.

[4] Tests were hurried on account of physical condition of subject.

TABLE 3—*Continued*

DISCRIMINATION BETWEEN UNEQUAL CIRCLES

Series	No. 17♀[2]						No. 18♂[2]						No. 20♂ (?)[2]						No. 21♀[2]					
	28+ 7+		28+ 12+		28+ 15+		28+ 7+		28+ 12+		28+ 15+		28+ 7+		28+ 12+		28+ 15+		28+ 7+		28+ 12+		28+ 15+	
	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W
1	10	0	10	0	8	2	8	2	10	0	8	2	9	1	8	2	7	3	7	3	10	0	10	0
2					9	1					5	5					7	3					7	3
3											8	2					[6]						8	2
4											[5]												8	2
5																							6	4
6																							10	0
7																								
8																								
9																								
10																								
11																								
12																								
13																								

[2] The record for ⊙ 28+ — ⊙ 7+ was preceded by 13 series in which the light factors were combined; the record given represents the first series after all factors except size were eliminated.

[5] Became discouraged.

[6] Tests ended on account of physical condition of subject.

Table 4, however, shows that this was not the case. The total average in seconds of time for ⊙ 28+—⊙ 7+ was 11. This time was increased for ⊙ 28+—⊙ 12+ to 16, and for ⊙ 28+—⊙ 15+ to 20 seconds.

TABLE 4

AVERAGE TIME FOR CHOOSING

Subject	⊙ 28+—⊙ 7+	⊙ 28+—⊙ 12+	⊙ 28+—⊙ 15+
6	15″	13″	7″
7	22	59	55
15	8	5	31
16	30	28	11
17	3	8	13
18	4	3	20
20	3	13	11
21	6	2	9
Average	11	16	20

It might be urged that this time average changed during the course of the experiments on account of the physical condition of the chicks. I do not believe this was the cause. Consider the record made by No. 21. This chick was one of the few which was turned out of doors, where its physical condition improved. That it was not weak at the conclusion of the work on size discrimination is suggested by the fact that it was put through nearly 1500 subsequent tests on form vision. This chick, as shown in table 3, made perfect records in ⊙ 28+—⊙ 15+ discrimination. It's time, according to table 4, averaged six seconds during the ⊙ 28+—⊙ 7+ tests, only two seconds in the ⊙ 28+—⊙ 12+ test, and nine seconds in the ⊙ 28+—⊙ 15+ tests. Evidently the chick was slower in the ⊙ 28+—⊙ 7+ tests where it was learning, its time was *not* increased when the variable was increased to ⊙ 12+ because the discrimination was still easy, but the time *was* increased when the more difficult discrimination, ⊙ 28+—⊙ 15+, was required.

The detailed behavior of the birds, furthermore, tends more firmly to establish the explanation offered. To illustrate how the behavior differed under the conditions of *easy* and *difficult* discrimination, two test sheets are presented.

TEST SHEET 1

Title of investigation, Size: $\odot$ 28+—$\odot$ 7+
Experimented on, 21
Harvard Psychological Laboratory, February 28, 1912
Record Sheet, 1. Series, 6

Test	Behavior	Record
1 l	// + /// "	O–4
2 l	+ $\overline{///}$ "	O–5
3 r	+ $\overline{///}$ "	O–2
4 l	+ $\overline{///}$ "	O–1
5 r	+ $\overline{///}$ "	O–2
6 r	+ $\overline{///}$ "	O–2
7 l	+ $\overline{///}$ "	O–2
8 r	+ $\overline{///}$ "	O–2
9 l	+ $\overline{///}$ "	O–2
10 r	+ $\overline{//}$''''''' "	O–10

10–0–3.2

Test sheet 1 shows that No. 21 lost no time on entering the discrimination chamber, but went *directly* in every case to the correct stimulus where it entered the electric compartment and escaped to the nest box. Its final record in this series was 10 correct and no wrong choices, with an average time of 3.2 seconds. Test sheet 2 shows a different condition. No. 16 was discriminating between $\odot$ 28+ and $\odot$ 12+. In the first test the chick was quiet for a long while on entering the discrimination box; then it went to the *wrong* side where it stopped momentarily about two-thirds of the distance between the back of the discrimination box and the beginning of the electric wires. It then turned to the left, went close to the left electric compartment, hesitated, and entered only to turn to the right and come out. After looking closely again at the *wrong* (right hand) stimulus it turned back to the *right* (left hand) compartment and escaped. The behavior was somewhat similar in the second test except that in turning away from the *wrong* stimulus which

TEST SHEET 2

Title of investigation, Size: ⊙ 28+—⊙ 7+
Experimented on, 16
Harvard Psychological Laboratory, March 6, 1912
Record Sheet 1; Series 16

Test	Behavior	Record
1 l	$\overline{///} - // + /// \vee - \overline{///} + ///$ „	O–47
2 r	$/// - // \underset{\frown}{\bigcirc} + ///$ „	O–38
3 l	$\overline{///} + - + // - \overline{/\ \triangle^1} + - // \triangle^1 + //$ „	E–2–58
4 l	$// + \overline{///} \wedge \underset{\frown}{\bigcirc} - /// + ///$ „	O–35
5 l	$/// + ///$ „	O–11
6 r	$/// - \overline{//}\ // \triangle^1 + ///$ „	E–1–21
7 r	$// + /// - /// \underset{\frown}{\bigcirc} + ///$ „	O–22
8 r	$+ ///$ „	O–5
9 l	$+ /// \overline{\underset{\frown}{\bigcirc}} - /// \overset{\smile}{\overline{\bigcirc}} + ///$ „	O–35
10 r	$// + ///$ „	O–10

8–2–28.2

this time was at the left, the chick turned itself completely around, counter clockwise, and came up to the *right* stimulus. The third test shows that No. 16 made two errors for each of which it was shocked. Test 9 indicates where the chick made two complete turns,—one counter clockwise from the *right* compartment which this time is at the left side, and one clockwise turn from the *wrong* or right hand compartment.

These two test sheets have been selected with an idea of showing as marked contrast as possible, that the reader may see how important is the chick's behavior during each test and series. The first shows that No. 21 had no difficulty in choosing the proper stimulus. The second indicates that No. 16 was comparing both stimuli and using considerable care in choosing, although it made two errors. The choice record would indicate poor discrimination, but the behavior shows that the bird was really discriminating. The time contrast also stands out in the

two series. These two series cannot be called typical of my experiments, yet, on the whole, they fairly represent the principles of the chick's mode of discrimination.

On the basis of choices, behavior, and time, it thus appears that the chick's limit in discrimination from a standard ⊙ 28+ lies somewhere between ⊙ 15+ and ⊙ 19+. To express the relation in terms of centimeters instead of square centimeters, the size in diameter of the variable is above 4.5 but below 5, that of the standard is 6. *The threshold of difference with a standard 6, therefore, is one-fourth to one-sixth.*

2. *Technique in experiments on size*

After working only a short time on size perception I was convinced that the chick which was being trained should be aided by having the differences between the two visual stimuli emphasized by a combination of light factors which would aid in the earlier discrimination. Two stimulus areas, differing from each other with respect to one quality only, present to the animal a problem which is quite unnatural. Under natural conditions it relies upon combinations of many visual factors. The perception of an isolated visual quality is a human problem, not an animal problem.

After a little preliminary sparring, therefore, the chick which was being trained was first presented with two stimuli differing from each other with respect to size, form, and brightness. The stimulus to be chosen was a triangle larger and brighter than a circle. On account of these conditions the *right* stimulus appeared in the lighter compartment. The chick was thus trained to choose (1) the lighter compartment in which the stimulus area was (2) larger, (3) triangular, and (4) brighter. The process now was to remove, if possible, so gradually that the chick was aware of no changes, all of the inequalities between the two stimuli except size difference. This was not difficult to do. As is indicated in table 3, undesired factors were removed before the 14th series, and the single record that is given in the table for ⊙ 28+—⊙ 7+ discrimination is the first one in which the conditions were so arranged that the chick could not possibly choose on the basis of any factor other than size. As the table also suggests this was the first and only ⊙ 28+—⊙ 7+ series of this sort; the next deals with ⊙ 28+—⊙ 12+ discrimination.

That the discrimination was not by some factor or factors other than size, an explanation of my method during the control tests will show. In the main, the same procedure, but a more crude form, was carried out with Nos. 3, 6, 7, for with them the control method was conducted less definitely from the beginning of the training.

TABLE 5
CONTROL SERIES FOR ⊙ 28+—⊙ 7+ DISCRIMINATION
NOS. 15, 16, 17, 18, 20, and 21
March 4, 1912
Record Sheet 1. Series 14

Test	Source distance from		General illumination of	
	+ Stimulus	— Stimulus	+ Compartment	— Compartment
	cm.	cm.		
1 l	240	125	Uncontrolled except for upper illumination.	
2 l	240	125	"	"
3 r	240	125	Much darker: Covered with 2 thicknesses of ground glass.	Much lighter: Covered with thin sheet of plain glass.
4 l	240	125	Darker: Covered with 1 thickness of ground glass.	Lighter: Covered with 1 thickness of plain glass.
6 r	240	240	"	"
7 l	180	180	"	"
8 r	180	180	"	"
9 l	180	180	"	"
10 r	180	180	"	"

The plan of the control is concisely stated in table 5. The location of the source lamps was varied according to the plan of the second and third columns. The illumination of the electric boxes was controlled according to the last two columns. In tests 1 and 2, the + or *right* stimulus area was considerably brighter and that compartment was markedly lighter than the other. In tests 3 and 4 the *right* stimulus was brighter but the general illumination was reversed. The *right* compartment was kept slightly darker after the fourth test than the *wrong* com-

partment. In this respect the conditions were exactly reversed from those of all former series, but the chicks had, in earlier series, become accustomed to reversal of brightness. It was under these conditions that the records of chicks 15, 18, 20, and 21, as presented in table 3, were made. It is incredible that the discriminations occurred on any basis other than size difference.

IV. FORM PERCEPTION

1. Literature

On account of the contrast between the outcome of my study of the chick's discrimination of form differences and the results reported by earlier experimenters in the same field, it is desirable to preface the account of my experiment with a brief review of these preceding studies on form perception. I shall limit this review to a consideration of two papers; one by Katz and Révész [9] and the other by Breed.[10] In both papers positive results have been reported.

In its simpler form, the "Klebmethode" of Katz and Révész consists in pasting on a cardboard, kernels of grain at which the chicken's pecking response is to become inhibited. Among these "glued" kernels is scattered a different kind of grain which the bird is allowed to pick up. To illustrate, it was found by the experimenters that the birds preferred rice to wheat. Twenty kernels of rice were glued to the background and 10 grains of wheat were scattered among them. A series was recorded every time the chicken picked up the wheat. The bird was classed as "Fehlerfrei" when it had picked up the wheat without pecking at the rice. The observers had two quantitative measurements for the rapidity of learning: (1) The number of series necessary for a perfect reaction; (2) the number of reactions to rice. To make sure that the "errorless" birds did not avoid the rice by means of the glue which might be visible on the pasted kernels, the rice was scattered loosely upon the cardboard in the same manner as the wheat. The wheat was again eaten by the "errorless" birds and the rice was left. The discrimination, the authors concluded, was between wheat and rice.

[9] Katz, D. and Révész, G. Experimentell-psychologische Untersuchungen mit Hühnern, *Zeit. f. Psych. u. Physiol. d. Sinnesorgane*, 1909, Bd. 50, P. 93.

[10] Breed, Frederick S. Reactions of chicks to optical stimuli, *Jour. Animal Behavior*, 1912, vol. 2, pp. 280-295.

Having found that the chicken could learn to pick up wheat and avoid rice, Katz and Révész sought to answer the question: By what means does the bird discriminate between the two kinds of grain? Accordingly they began to test the chicken's discrimination of sizes and forms. Because the chickens could be trained to eat only half-grains of rice when scattered among whole grains, the authors were convinced that there was a discrimination of size and form. A little further study leads them to conclude: "The chicken also discriminates between squares and triangles. Knowledge of this fact we secured through a variation of the experimental procedure. Out of green peas, (some of which had been readily eaten), we cut three and four cornered pieces. On account of their moisture they could not be glued down, so we laid the four sided-pieces upon a glass plate and the three-sided pieces under it. The chicken found that the three-sided pieces could not be reached and soon ceased to peck at them. Then if we laid both forms upon the glass plate, only the squares were picked up. By means of the same method we found that the chicken discriminates between triangles and circles as well as squares and triangles."

The study made by Katz and Révész is open to the same general objection with which this paper was opened. They have, in too many cases left the reader uncertain of the exact conditions. They have tried to do too much and have not accomplished any one task; their report indicates carelessness and indifference to details. Such work makes an interesting paper. It is probably received more favorably by the majority of readers than an intensive study of a problem. After all, however, this sort of superficial work does not get us anywhere. No doubt the statement that "the chicken also discriminates between squares and triangles" and "between triangles and circles as well as squares and circles," holds true for the conditions under which the experiment was made. But from the written account *one cannot tell what the conditions were.* One does not know, for example, the relative sizes of the different forms. Were they equal in area? Was the square an inscription of the circle or *vice versa?* Was the diameter of the circle equal to the height of the square or the altitude of the triangle? These are some of the factors which *must be known* before one can safely say that the chicken perceives form. One could

truthfully state that an animal was discriminating between a circle and a triangle if it regularly chose the former even though the triangle were *seven* times as large as the circle, but it is quite improbable that the basis of discrimination in such a case would be anything other than size. Since such points were omitted from the written account, one suspects that the technique of the experimenters was decidedly imperfect. Even had they taken the precaution of equalizing the sizes, they would then have faced a problem still more difficult. Their task would then have been to show that the discrimination was not due to unequal stimulation of different parts of the retina. Furthermore, no information is offered as to the method of cutting these forms. It is a very difficult matter to cut green peas after any regular manner. How can the observers be certain that their subjects discriminated on the basis of form rather than irregularities in the surfaces? They make no mention of "check tests" to eliminate this possibility. It is because they leave vital points like these unmentioned and apparently unnoticed that one is led to class the experiment as a very superficial piece of work.

In the report by Breed on the reactions of chicks to form stimuli [11] the conditions of the experiment have been accurately presented. His chicks were given an opportunity to select a circle when appearing along with a square. Both stimuli were presented in a dark room by means of the illumination of screens consisting of two plates of flashed opal glass, over which were set mats of tin or cardboard containing the desired openings. Three chicks were used in this study, one of which, No. 76, "learned to discriminate two optical stimuli on the basis of difference of form."

Because his studies of the reaction of other chicks to similar stimuli yielded negative results, Breed attributes the positive reactions of No. 76 to a fortunate choice of subject. Unfortunately, however, he seems to have made no control tests to determine whether or not the distribution of light on the chick's retina was influential. An inversion of a square would cause no change in the distribution of light; such a change might have been produced by turning the square through 45°. A control test of this sort, however, is more easily made when a

[11] *Op. cit.*, pp. 290-293.

triangle is presented along with a circle. Inversion of a triangle produces a marked difference in the distributions of the light which reaches the retina, yet the form of the stimulus is unchanged.

In the following report of my own tests on form discrimination, it will be noticed that I obtained results as positive as those of Breed when the apex of the triangle was at the top. When the position of the triangle was inverted, however, the perfect reactions ceased. It was so difficult to get a chick to react positively when a square was used with a circle that I found it practicable to make the control test on the distribution of light only in the case of the circle-triangle reaction. It is regrettable that Breed failed to make this control test in his experiments.

2. Experiments

The development of my system in the study of the chick's form perception followed, in general, that which occurred in my study of reactions to sizes. Work on this visual factor, i.e., the discriminative ability between circles and triangles which are equal in area, was done with two of the second group of chicks, Nos. 9 and 11. The latter became afflicted with "weak legs" after the 24th series, up to which time it had given no positive results. The results from No. 9 might be regarded as slightly positive.

On the whole the results in table 6 show a preference for the circle. Series 10 was perfect. One contains 90% of *right* choices and several reached 70% or 80%. It is not surprising that the chick became discouraged, since I had not yet adopted the plan of beginning with complex stimuli and working toward the simple. A system of control similar to that described under size perception (p. 98) was used throughout the work. The surprising feature is the high percentage of right choices on January 4 and 5 when the chick was becoming discouraged and frightened. This condition resulted in a rush for one stimulus or the other as though "to get the choice over." It appears that there was some sort of a difference between the two illuminations which tended to catch the eye of the rushing chick, but this was not clearly enough perceived for a dependable basis of discrimination. Finally, this experiment did not definitely prove, even though we admit a preference for the circle,

TABLE 6

FORM PERCEPTION: ⊙ 28+—△ 28+

No. 9. Hatched: December 1, 1911. Sex: ♀

Series	Date	Right	Wrong	Time
1	Dec. 9	4	6	....
2	11	8	2	44.2
3	12	5	5	47.1
4	13	6	4	49.2
5	14	7	3	24.8
6	15	6	4	54.6
7	16	6	4	28.3
8	18	4	6	56.8
9	19	5	5	15.9
10	19	10	0	29.5
11	20	6	4	60.2
12	21	8	2	16.7
13	22	7	3	42.7
14	23	5	5	32.8
15	25	6	4	29.5
16	26	8	2	24.2
17	27	7	3	42.8
18	27	4	6	32.8
19	28	4	6	44.7
20	28	6	4	33.2
21	28	5	5	22.
22	29	3	7	29.3
23	29	6	4	13.3
24	30	7	3	12.7
25	Jan. 2	9	1	27.8
1	2	7	3	30.9
2	3	5	5	25.4
3	4	7	3	12.5
4	5	8	2	4.6 (Rushing)
5	6	(Discouraged; rushed blindly to either stimulus.)		

that the chick perceived form. The preference may well have been due to the distribution of light or the unequal stimulation of different parts of the retina.

As has been earlier stated, the original plan was to test the minimum form difference a chick could perceive. The result of my investigation with No. 9 convinced me that it was necessary first to determine whether or not the chick perceives form. When the study of form perception was begun with group 3, the plan that was explained in the preceding chapter on size was adopted. That is, form, in the beginning, was made only one of other visual factors which were gradually eliminated as the experiment progressed. Only one chick, No. 21, gave anything like positive results. This bird, including 22 series on

size and brightness vision, was given more than 1500 tests. The work done with it is the only experiment of value that was made on form perception.

The experiment on form discrimination with No. 21 may be divided into three parts: (1) A preliminary investigation; (2) reactions to the triangle-circle and (3) reaction to the circle-triangle. When the work on form perception was begun, I had learned how to get much more work out of my subject and, at the same time, less energy was called forth than in the earlier tests. No. 21 knew me quite well by this time. It was perfectly contented to leave the other birds in the chick-room and go alone with me to the dark-room for work. The chief reason for this was the fact that it always had its morning meal in the dark-room. As soon as we reached the experiment room it was fed a little chick food and allowed to run freely about until the apparatus was made ready for the tests. While I was preparing the apparatus, the chick would follow me about the room (then lighted) twittering and contented, but if I left it alone for a few minutes its dissatisfaction was made known by loud and persistent peeping.

When everything was ready the chick was placed in the apparatus. At this point the note in its voice changed very noticeably. I cannot describe this sound, except that it was a slightly modified, I believe it might be called a *modulated*, "hovering twitter." Very commonly this peculiar sort of singing changed as the bird entered the discrimination chamber, to the "food twitter" which was continued all the time it was inspecting and comparing the two stimulus areas and, of course, after it had entered the nest box for there it was rewarded by finding a few grains scattered in the litter. In this manner a series could be completed in about 15 minutes after which the chick was taken from the experiment box and given more food and its freedom in the room. This procedure could be carried out until the chick's hunger was satisfied after which the tests went so slowly and with such uncertainty that it was found best to postpone the work until the following morning. As the bird became older this mode of experimentation could be carried on as long as three hours.

While I was preparing test sheets or taking notes at my desk between experiments, No. 21 would crowd up about my

feet and beg to be taken up. If it were allowed to perch itself on my arm it would sit there as long as I was quiet, contentedly preening its feathers and occasionally giving a short "hovering twitter."

From these facts, I am led to conclude that the chick really "enjoyed" the experiments.

Table 7 shows the results of my first study of form with No. 21. The chick was trained to go to the triangle and to reject the circle or square. While none of the results here recorded are clear cut, there is strong evidence that the chick was discriminating between the two stimuli. This experiment was hurried lest the chick should not remain in good physical condition, but at its conclusion the bird was in excellent health so the work was repeated with much more thoroughness.

TABLE 7

FORM PERCEPTION

No. 21. Hatched: February 9, 1912. Sex: ♀

Discrimination	Series	Date	Right	Wrong	Time
△ 28+—⊙ 7+	6	March 13	7	3	10″
" —⊙ 12+	7	" 13	9	1	11
" —⊙ 15+	8	" 14	8	2	18
" —⊙ 19+	9	" 14	4	6	12
" — "	10	" 14	6	4	8
" — "	11	" 14	8	2	33
" — "	12	" 14	8	2	12
" — "	13	" 14	8	2	12
" — "	14	" 14	8	2	12
" —⊙ 23+	15	" 14	8	2	13
" — "	16	" 15	8	2	6
" —⊙ 24+	17	" 15	8	2	13
" —⊙ 25+	18	" 15	9	1	6
" —⊙ 26+	19	" 15	6	4	17
" —⊙ 27+	20	" 15	8	2	18
" —⊙ 28+	21	" 15	7	3	17
" — "	22	" 16	9	1	8
" —□ 28+	23	" 16	9	1	40
" —⊙ 63+	24	" 18	6	4	56
" — "	25	" 18	6	4	85

The last two series (24 and 25) of table 7 were introduced as control tests. The circle was larger than the triangle, hence it afforded an opportunity to see if the chick would react to form difference, after this training, rather than size difference. Apparently it was demanding too much of the chick, for No. 21 be-

came discouraged at this point and the training had to be repeated.

In table 8 is presented the results of the re-training of No. 21. While the record, as shown in the table, is quite convincing

TABLE 8

FORM PERCEPTION: △ 28+—⊙ 28+

No. 21. Hatched: February 9, 1912. Sex: ♀

Discrimination	Series	Date	Right	Wrong	Time
△ 28+—⊙ 9+	9	March 20	10	0	6
" —⊙ 12+	10	" 20	10	0	14
" —⊙ 15+ (inscription of triangle)	11	" 21	9	1	9
" — "	12	" 21	10	0	9
" — "	13	" 21	10	0	8
" — "	14	" 22	10	0	6
" —⊙ 19+	15	" 22	5	5	15
" — "	16	" 23	9	1	6
" — "	17	" 23	10	0	6
" —⊙ 23+	18	" 23	9	1	7
" — "	19	" 25	9	1	20
▽ 28+—(inverted)—⊙ 23+	20	" 25	5	5	92
△ 28+ (upright)—⊙ 23+	21	" 25	8	2	20
" — "	22	" 26	10	0	17
" —⊙ 25+	23	" 26	10	0	12
" —⊙ 27+	24	" 26	8	2	12
" — "	25	" 26	9	1	6
" — "	5	" 27	8	2	35
" — "	6	" 27	6	4	8
" —⊙ 28+	7	" 27	8	2	9
" — "	8	" 28	10	0	5
" — "	9	" 28	10	0	9
△ 11+ (inscribed)—⊙ 28+	10	" 28	6	4	40
△ 28+—⊙ 28+	11	" 28	10	0	15
" — "	12	" 29	9	1	5
▽ 28+ (inverted)—⊙ 28+	13	" 29	2	5	83
△ 28+ (upright)—⊙ 28+	14	" 29	9	1	7
" — "	15[1]	" 30	7	0	6
" — "	16	" 30	9	1	7
" — "	17	" 30	10	0	24
" — "	18	April 1	10	0	15

[1] In series 15 the triangle was inverted during tests 5–7; the result was 2 wrong choices with an average time of 39 seconds.

that the chick discriminated between the triangle and the circle, it is, nevertheless, equally convincing that the chick did not perceive the form difference. It had reacted properly to the △ 28+—⊙ 19+ and —⊙ 23+ when the apex of the triangle was at the top, but when the triangle was inverted (series 20,

March 25) the animal did not choose the triangle more than half of the time. With the triangle again upright, the correct reactions returned. Proper responses were secured when the areas were equal, (series 8 and 9, March 28), but when the inscribed triangle was substituted for the standard, the chick was again confused. Series 13, (March 29), another case where the triangle was inverted, gave more decided negative results which were immediately preceded and followed by almost perfect reactions. As a further test of the effect of inverting the triangle, the inversion was made during a regular series, 15, (March 30). The result of the inversion during tests 5–7 was one right choice, out of 3 chances with an average time of 39 seconds. These three special tests were preceded and followed during the same series with *perfect* tests the time of which averaged *less than one-third* that of the special tests.

In connection with these results, I present in detail the behavior of No. 21 when it was given △ 28+—⊙ 63+ discrimina-

TEST SHEET

Title of investigation, Form: △ 28+—⊙ 63+
Experimented on, 21
Harvard Psychological Laboratory, March 18, 1912
Record Sheet, 2; Series, 24

Test	Behavior	Record
1 l	$/// - \overline{///} + \overline{///}$ ○ $- \overline{\overline{///}} + ///$ ▪	O-65
2 r	$/// + ///$ ▪	O-7
3 l	$/// - \bar{/}\bar{/}\bar{/}$ $\triangle^1 + ///$ ▪	E-1-14
4 l	$/// - /// + /$ ʅʅ $+ ///$ ▪	O-14
5 l	$/// - ///$ $\triangle^1$ $\overline{''''''''}$ $+ ///$ ▪	E-1-55
6 r	$/// - + ///$ $\overline{'''''''''}$ $- ///$ ○ $+ // - \bar{/} + /\bar{/}$ $\overline{''''''}$ $- \triangle^2 +$ ▪	E-2-171
7 r	$/// + // - // + // +$ $\overline{''''''}$ $+ // -$ $\overline{'''''''}$ $+ ///$ ▪	O-94
8 r	$// + //$ ʅʅ $- //$ $\overline{\overline{ʅʅ}}$ $\overline{''''''}$ $+ //$ ▪	O-69
9 l	$/// + ///$ ▪	O-4
10 r	$/// - /$ ○ $+$ $\overline{\overline{ʅʅ}}$ $- / + /$ $\overline{'''''}$ $- //$ $\triangle^1 + //$ ▪	E-1-70

6-4-56.3

tion. These tests are reported in table 7 as series 24 and 25. The test sheets show how utterly confusing to the chick was this condition. The results of this test were verified in the series recorded in table 8 under date of March 28 where the record is the same and the behavior was very similar.

Finally this chick's response to the circle-triangle was tested. The reversion of the condition for discrimination was at first confusing to the chick, and much patience on the part of the experimenter was necessary. The plan now was to begin with

TEST SHEET

Title of investigation, Form: △ 28+—⊙ 63+
Experimented on, 21
Harvard Psychological Laboratory, March 18, 1912
Record Sheet, 2; Series, 25

Test	Behavior	Record
1 r	/// + // − /// ○ \\ − // + // − // ⊙ + / ○ − / △⁰ + // "	E–0–145
2 r	/// + // ○ ″″″″ − /// + / ○ − // ○ + /// "	O–79
3 r	/// + − ○ + // ○ ″″″″ + /// "	O–45
4 r	/// ○ + /// ○ ″″″″ − / + /// ″″″″ + /// "	O–60
5 l	/// ⊙ − // + /// − /// ″″″″ + /// − // △¹ + /// "	E–1–61
6 l	// + /// − / ○ ″″″″ + /// "	O–33
7 l	// + // − /// ″″″″ − /// ○ − // + /// "	O–46
8 l	/// + /// − ○ ″″″″ + / − /// △¹ + /// "	E–1–35
9 r	/// − /// ○ ″″″″ − // ″″″″ + /// "	O–99
10 l	/ − / ″″″″ − △⁰ ″″″″ − △⁰ ″″″″ − △¹ + "	E–1–243
		6–4–84.6

⊙ 63+—△ 28+ and gradually to eliminate the size difference by substituting successively smaller circles. In this case the *right* stimulus area was the variable and the standard was the sign of "shock" and "no escape." From the tabulation of these results the early records are omitted since these tests were only

made as a means of getting a discrimination between the later forms. The size of the circle was gradually decreased from ⊙ 63+ to ⊙ 38+ at which point the tabulation begins. About 400 preceding tests were necessary to bring the bird to the point in its training at which the records of the table commence.

Table 9 shows that the chick was able to acquire the circle-triangle habit. The plan of controlling the qualities other than form was carried out as heretofore explained. After No. 21 had acquired the ⊙ 28+—△ 28+ reaction, the order shown in the table was followed to determine whether or not the bird

TABLE 9

FORM PERCEPTION: ⊙ 28+—△ 28+ and ⊙ 28+—□ 28+

No. 21. Hatched: February 9, 1912. Sex: ♀

Discrimination	Series	Date	Right	Wrong	Time
⊙ 38+—△ 28+	12	April 20	10	0	4
⊙ 33+— "	13	" 20	6	4	7
" — "	14	" 20	5	5	7
" — "	15	" 20	7	3	5
" — "	16	" 20	8	2	8
" — "	17	" 20	6	4	6
" — "	18	" 20	10	0	8
⊙ 28+— "	19	" 20	10	0	5
⊙ 44+— "	20	" 20	10	0	5
" —▽ 28+ (△ inverted)	21	" 20	9	1	5
⊙ 28+—△ 28+ (△ upright)	22	" 21	5	5	6
" — "	23	" 21	6	4	4
" — "	24	" 21	6	4	7
" — "	25	" 21	3	7	6
⊙ 33+— "	1	" 21	10	0	4
" — "	2	" 21	10	0	4
⊙ 28+— "	3	" 21	10	0	5
" —▽ 28+ (△ inverted)	4	" 21	6	4	5
⊙ 15+—△ 28+ (⊙ inscribed)	5	" 21	4	6	11
⊙ 28+— "	6	" 22	8	2	10
" — "	7	" 22	8	2	7
" — "	8	" 22	10	0	6
" — "	9	" 23	8	2	5
" — "	10	" 23	8	2	5
" — "	11	" 23	9	1	4
" — "	12	" 23	9	1	7
" — "	13	" 24	9	1	5
" — "	14	" 24	9	1	4
" —□ 28+	16	" 24	5	5	5
" — "	17	" 24	5	5	3
" — "	18	" 25	3	7	4
" — "	19	" 25	6	4	4
" — "	20	" 25	5	5	6
" — "	21	" 25	8	2	6
" — "	22	" 25	5	5	4

actually depended upon form difference. If it had a perception of form such as has been found to exist for size, then no marked change in the results should have occurred when ⊙ 28+ was replaced by a circle that circumscribed or inscribed the standard triangle; and if the chick perceived three-sidedness or triangularity on the one hand and circularity on the other hand, there should have been no important modification of its reaction when the triangle was inverted.

The work on form thus indicated that the chick can discriminate between circles and triangles of equal area, and there are indications of a discrimination between circles and squares of equal area (witness series 21, April 25, table 9, and series 23, March 16, table 7), but with the application of control tests we have indications that *this discrimination is on some basis other than form.* The results from the inversion of the triangle indicate that the basis of choice depends upon the unequal stimulation of different parts of the retina. When the extended base of the triangle is so placed as to stimulate the region of the retina which was formerly stimulated by the apex of the triangle, the chick becomes confused. Under the conditions of the present experiment, therefore, I am forced to conclude that *the apparent reactions to forms are the result of keen perception of size differences.*

V. RELATIVE VALUE OF SIZE, FORM, AND BRIGHTNESS

The preceding study of size and form can leave no doubt as to their relative importance in the chick's visual life. On the basis of trial and error it has been shown that the number of tests to produce any approach towards a perfect response to the circle-triangle is vastly greater than the necessary number in the case of the large-small discrimination. This study has also revealed the fact that even after perfect reactions to the circle-triangle were established, the form element played no part in the discrimination. The size element, under these conditions, is *the* important factor for the chick, and *form in the stricter sense* has been found to have no discriminative value.

A brief study was made to determine experimentally the relative importance of size, form, brightness, and general illumination. The subjects used were Nos. 15, 16, 17, 18, 20, and 21. They were trained to go to a triangle which had a greater

area and was brighter than the simultaneously presented circle. The triangle also appeared in the compartment which was more highly illuminated than that in which the circle was presented. After the chick had learned to choose the triangle without any errors, all but one of these factors were eliminated. It was thus determined which factor had the greatest value for the chick.

This method, I clearly recognize, is not reliable. It was only adopted as a means of securing preliminary information on this particular problem. The most important defect which may be present is that it provides no certain way of ascertaining the threshold values for each visual factor. For example, a difference of 11 sq. cm. between the areas of the two stimuli may be high above the chick's threshold of difference, whereas the difference between a triangle and a circle may be scarcely above this liminal threshold. I had no way of equalizing the four factors for discrimination. Subsequent work such as that reported on size and form must determine these values for all of the "light" elements on the basis of right and wrong choices.

The amount of difference in brightness and general illumination was that which, under the mechanical conditions previously described, would result when the *right* source lamp was placed 125 cm. from its stimulus area and the *wrong* lamp was 240 cm. from its display surface. This caused a marked difference to the human eye both in brightness and general illumination. Table 10, however, shows that it was of little significance to the chick. According to the averages, size was the first factor; *right* choices depending upon it alone amounted to 86%. Brightness and general illumination combined stand next with nearly 70% of *right* choices, and form stands last with the *right* and *wrong* choices nearly equal. Considering these results in connection with those previously presented on size and form, it seems highly evident that *the relative values of light factors for the chick's vision is respectively size, brightness and general illumination, and form.*

VI. GENERAL IDEA

The frequent recurrence of discussions relative to the general idea in connection with animal studies has had the effect of keeping before me, during the present study, the question whether or not the chick has a general idea of sizes and forms. Accordingly, after several subjects had been trained to make

TABLE 10

RELATIVE VALUES OF VISUAL FACTORS

Condition of Discrimination	Series	Date	Subjects																		Average		
			15			16			17			18			20			21					
			R	W	T	R	W	T	R	W	T	R	W	T	R	W	T	R	W	T	R	W	T
△ 28+ brighter and in lighter compartment than ⊙ 7+	1	Feb. 20	10	0	34	8	2	40	9	1	16	8	2	40	7	3	40	9	1	10	$8\frac{1}{2}$	$1\frac{1}{2}$	30
	2	22	10	0	8	8	2	10	9	1	10	10	0	5	7	3	33	9	1	18	$8\frac{5}{6}$	$1\frac{1}{6}$	14
	3	24	9	1	8	8	2	21	10	0	7	10	0	8	8	2	12	10	0	5	9	1	$10\frac{1}{6}$
	4	26	10	0	3	6	4	35	10	0	6	9	1	4	9	1	10	10	0	3	9	1	$10\frac{1}{6}$
	5	27	8	2	11	7	3	24	9	1	22	7	3	16	9	1	21	6	4	12	$7\frac{2}{3}$	$2\frac{1}{3}$	$17\frac{2}{3}$
	6	28	9	1	7	9	1	7	10	0	6	10	0	4	10	0	7	10	0	3	$9\frac{2}{3}$	$\frac{1}{3}$	$5\frac{2}{3}$
	7	29	9	1	14	9	1	4	10	0	5	9	1	6	10	0	8	10	0	2	$9\frac{1}{2}$	$\frac{1}{2}$	$6\frac{1}{2}$
	8	29	10	0	3	9	1	9				10	0	2							$9\frac{2}{3}$	$\frac{1}{3}$	$4\frac{2}{3}$
	9	Mar. 1	10	0	5	10	0	20				10	0	7							10	0	$10\frac{2}{3}$
	10	1				10	0	12													10	0	12
Form and size	11	1–2	8	2	8	9	1	22	6	4	4	8	2	10	10	0	4	9	1	2	$8\frac{1}{3}$	$1\frac{2}{3}$	$8\frac{2}{3}$
Form	12	2	3	3	—	3	3	—	3	3	—	2	4	—	4	2	—	4	2	—	$3\frac{1}{6}$	$2\frac{5}{6}$	—
All factors combined	13	4	10	0	6	9	1	45	10	0	4	7	3	36	10	0	4	10	0	3	$9\frac{1}{3}$	$\frac{2}{3}$	$16\frac{1}{3}$
Size	14	4	9	1	7	9	1	30	10	0	3	8	2	4	9	1	3	7	3	6	$8\frac{2}{3}$	$1\frac{1}{3}$	$8\frac{5}{6}$
Brightness and general illumination	15	5	5	5	9	6	4	26	9	1	5	7	3	3	7	3	7	7	3	5	$6\frac{5}{6}$	$3\frac{1}{6}$	$9\frac{2}{3}$

perfect reactions to the condition ⊙ 28+—⊙ 12+, I tested, without further training, their reactions to two different circles the sizes of which bore the same relation to each other as that of the former pair. This test was made for two different pairs of circles to determine whether or not a chick trained to choose the larger of two stimuli will choose the larger of two other stimuli that are larger or smaller than, but have the same proportion in size as those with which it was originally trained.

The diameters of the circles used in ⊙ 28+—⊙ 12+ discrimination are respectively 6 cm. and 4 cm. The variable, therefore, is two-thirds of the standard. After the chick had reacted perfectly to the 6-4 circles, they were replaced with two circles having diameters respectively of 4 cm. and 3 cm., *i.e.*, having the same difference ratio. In the next place they were replaced with two 9-6 circles in which case the difference ratio again remained unchanged. All of the subjects, except No. 3, mentioned in the foregoing discussion, were tested in this manner.

The results of these tests for general ideas are briefly as follows: a chick which has been trained to choose a 6 cm. circle and *reject* a 4 cm. circle will *choose* the latter when presented with a 3 cm. circle. Likewise, it will choose a 9 cm. circle when presented with a 6 cm. circle. In the one case, what has formerly been the sign for a negative reaction is accepted as the sign for a positive reaction. In the other case, what has previously been the positive sign is rejected, when presented with a larger stimulus, as a "shock sign."

VII. SUMMARY

Under the conditions of the present method, the chick's threshold of difference in size perception lies between one-fourth and one-sixth when the diameter of the standard circle is 6 cm.

Earlier experimenters on the chick's perception of forms have failed to eliminate all possible conditions for discrimination other than the factor of form. The chick can discriminate between circles and triangles and circles and squares which are equal in area, but, with the conditions as described in this paper, none of the subjects with which the present experiment has been conducted were able to discriminate between visual stimuli on the basis of form alone. Reactions to optical stimuli which have been interpreted by observers as indicating form discrimination are probably made on the basis of unequal stimulation of different parts of the retina. If local inequality of excitations on the retina be the basis of these reactions, then the apparent discrimination of form by the chick is, in reality, a keen perception of size differences.

In sharp contrasts with the reactions to form stimuli are the responses to sizes. A chick can acquire a perfect circle-triangle reaction, but control tests show that it has no general idea of circularity in contrast with triangularity. On the other hand, a "large-small" trained chick reacts positively to the larger of two stimuli even though this particular stimulus had been the "shock" stimulus in previous experiments.

The order of importance of factors in the chick's vision is size, brightness and general illumination, and form.

No. 16. 289

REACTIONS OF CHICKS TO OPTICAL STIMULI

FREDERICK S. BREED

The University of Michigan

From the Harvard Psychological Laboratory

One figure

In the course of a previous study,[1] in which the rapidity of development, permanency, and interrelation of certain habits in barred Plymouth Rock chicks were tested, the problem suggested itself of determining more accurately the nature of some of the optical stimuli in response to which a chick is able to acquire a habit of reaction. In these earlier experiments the animals were tested in their ability to discriminate two simultaneously presented stimuli differing essentially in color, form, or size. Although certain conclusions as to the *what* of discrimination seemed quite safe, it was thought best to conduct a further investigation in this direction with greater refinement of method. The earlier tests seemed to yield positive results in the discrimination of color and size differences, and negative results in the tests on forms.

The problem, then, became that of determining more accurately whether or not the elements of form, size, and color may become the basis of discrimination for the chick. The work was undertaken and completed in the Harvard Psychological Laboratory under the guidance of Professor Robert M. Yerkes, who proposed the investigation and assisted throughout with invaluable suggestions.

APPARATUS

A perspective of the apparatus employed is shown in figure 1. For a complete description, including dimensions of parts, the reader is referred to an article by L. W. Cole in an earlier number of this journal.[2] Following Cole's description, we may consider the apparatus as composed of three boxes: (1) The hover box, O; (2) the illumination box, covered by the

[1] Breed, F. S. The development of certain instincts and habits in chicks. *Behavior Monographs*, vol. 1, no. 1, 1911, p. 41 ff.

[2] Cole, L. W. The relation of strength of stimulus to rate of learning in the chick. *The Journal of Animal Behavior*, vol. 1, no. 2, 1911, p. 112 ff.

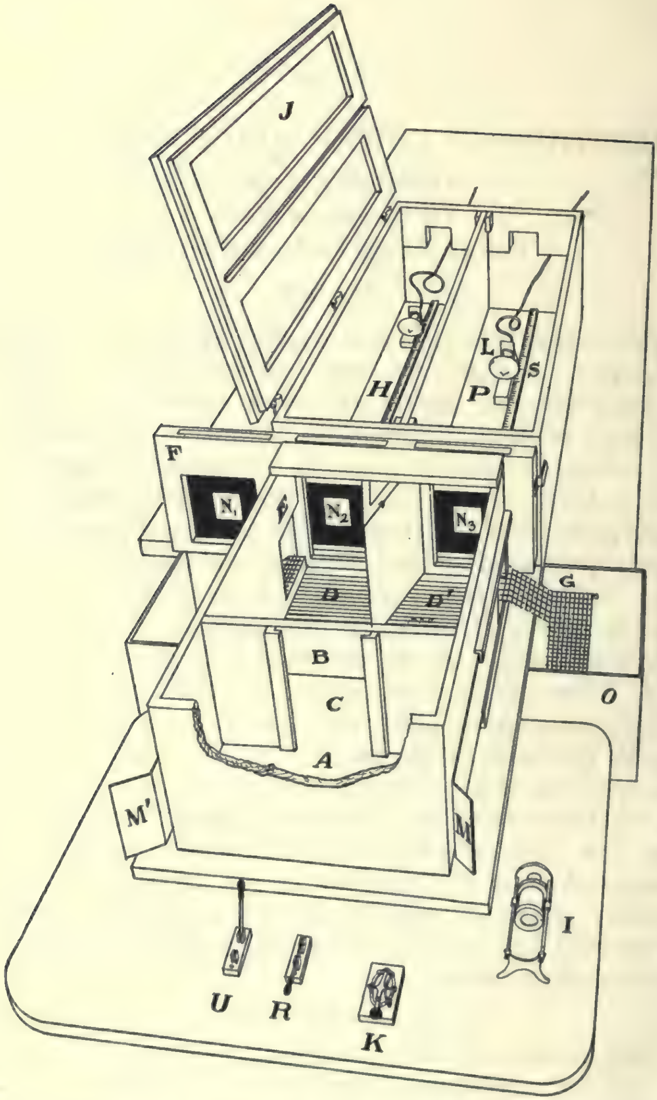


FIGURE 1. Perspective of apparatus used in testing reactions to colors, forms, and sizes.

1. *Hover box*, O; G, inclined planes of wire mesh leading from exits E, of the experiment box to the hover box.

2. *Illumination box.*—H, left compartment; P, right compartment; L, stimulus lamps; S, metric scales.

3. *Experiment box.*—A, entrance compartment; C, discrimination compartment; B, gateway between A and C; D and D′, electric passageways; N_1, N_2, N_3, glass areas for illumination in the sliding frame F; M and M′, cardboard shutters for closing exits E; U and R, electric keys for separate control of lamp in O and reading lamp (not represented in drawing) on outside of A, above R; K, stimulus key; I, inductorium.

The figure is symmetrical, hence the letters G, E, L, and S designate both the parts which they respectively mark and the duplicates of these parts on the opposite side of the figure.

lid J, containing the source lamps for the stimuli; and (3) the experiment box, divided into the two compartments, A and C. The illumination box and the experiment box were separated by a sliding frame F, containing three openings each 12 x 12 cm., in which were mounted the desired screens, N_1, N_2, N_3.

The point of the partition separating the electric passageways D and D′ was 31 cm. from the nearer ends of the metric scales.

In some respects a different adjustment of the apparatus from that used by Cole was employed by the writer. A 2 c. p. electric lamp instead of a 16 c. p. was located in the hover box. The inclined planes G, which Cole discarded, had been satisfactorily used throughout the present investigation. It may be more natural for a chicken to run up hill than down, as Cole intimates, but it is not quite so clear why the reader is asked, in explanation of the difficulty of descent, "to imagine a man descending a steep incline with his body leaning far forward." The difference in the results with this device is no doubt partly explained by the dark-room conditions maintained throughout the present test. The animals were attracted to the lamp in the hover box and did not wander off into the darkness after they reached it.

The source lamps in the illumination box were two turnip-shaped reflectors, having the lower half of the bulb frosted and the upper half mirrored. When operated at 110 volts, the end-on candle power, as measured at a distance of 3.048 meters, was 35.4 and 39.4 for no. 1 and no. 2, respectively, at the termination of the experiments. These measurements were made by the Electrical Testing Laboratories, 80th Street and East End Avenue, New York City.

The feet of the chicks were kept damp by a wet pad of cloth in the gateway B. This condition of the feet of the chicks insured greater regularity of the shock upon contact with the electric wires.

The experiments were all conducted in a dark-room. The apparatus within and without was painted a dull black. As a further precaution the discrimination compartment C, of the experiment box, was covered with a black cardboard hood so arranged that an opening above B was left of sufficient size to permit the experimenter to observe the behavior of an animal

in the presence of the stimuli. The black cardboard shutters M and M′ were kept closed over the exits E until the animal had selected one or the other of the two stimuli. Thus the only light that reached a chick while in the act of discrimination was transmitted through the glass screens mounted in the sliding frame F. In the size and form experiments these screens consisted each of two plates of flashed opal glass, depolished toward the lamps, glossy-surfaced toward the chick. The illuminated areas of various sizes and shapes were produced by black mats of tin or cardboard that contained the appropriate openings and were set in the frame over the glass on the side away from the lamps. In the color work colored glass was inserted. The frame was adjusted on brass rollers that were constructed so as to work noiselessly. The lamps in the illumination box, mounted as they were on moveable cast-iron bases, could be shifted back and forth along the metric scales to provide such variation in the relative brightness of the stimuli as the demands of the experiment dictated.

The dark-basket mentioned in the following account was a willow basket lined with black cloth and covered with the same material. It was used for the purpose of dark-adapting the chicks. The animals were kept in this receptacle before and between trials during all the form and size work, and during all the color work except the preliminary tests.

METHOD

A chick that was being tested was introduced into the apparatus by hand at A, whence it passed through the gateway B, and walked toward one of the two lighted areas. If an error was made, the animal received an electric shock, controlled at K by the experimenter, whereupon it soon learned to pass to the other side of the compartment and secure its release. If a correct selection was made, the shutter M or M′ was withdrawn, exit E was thereby slightly illuminated with the dim light reflected from the hover box, and the animal found its way down the inclined plane G to heat, food, light, and water. There was a possible source of error in the method at this point against which the operator had to take precaution constantly. He made it a rule not to withdraw the shutter until a chick was well onto the wires in a position to be shocked. Occasionally a chick would

stand near the edge of the electric wires and crane its neck toward the exit. It is easy to see how a slight noise on the proper side or a slight movement of the proper shutter might have precipitated a correct reaction when no discrimination of the important stimuli had taken place. The method was open to improvement at this point. So far as possible, chances for the involuntary interference of the experimenter should be eliminated. The writer was at work on an automatic device to meet this difficulty at the time these experiments were brought to a close.

The tests were given in series of ten trials each. In each series there was an equal distribution of the two stimuli on the right and left sides of the apparatus. These changes of position were handily produced by shifting the sliding frame, which contained like mounts at either end and the one to which these were opposed in the central opening. It is readily seen how a shift of the frame altered the relative positions of the two stimuli as well as changed the source lights for each.

The order of procedure was preference tests, training tests, and control tests. The first consisted of one or more series in which the chosen stimuli were alternated right and left and no electric shock was used. In the color work certain "preliminary" tests were made which were really of the nature of preference tests on various combinations of stimuli. In the training tests the right and left shifts were equally balanced but varied in order. In the control tests the shock was dispensed with.

REACTIONS TO COLORS

In the previous color tests, where the reflection method was used and black and blue were opposing colors, chicks not only formed the habit of rejecting blue but continued this rejecting reaction both when white was substituted for black and when a different tint of blue was substituted for that used in the original training. It seemed very probable that difference in color was the basis of discrimination. The experiment reported below was an attempt to test the color discrimination of the chicks by the transmission method. The colors chosen for this experiment were red and blue. During the course of the preliminary tests the following setting of the screens was adopted for the preference tests and the later training: Two red screens, one at either end

of the sliding frame, each composed of two plates of red glass cut from the same sheet; one blue screen, in the middle of the frame, made up of three plates of blue and violet glass. These color names represent simply what the colors appeared to the experimenter to be in advance of spectroscopic analysis. In each of the three screens the colored plates were covered on the side toward the chicks with a plate of opal glass, and over this was set a black mat with a circular opening 8 cm. in diameter. A spectroscopic analysis of the light transmitted by these screens was kindly made for the writer by Professors Yerkes and Cole, which showed the range of red to be from 760 to 640$\mu\mu$, and that of the blue-violet from 480 to 430$\mu\mu$.

That young chicks are positively phototropic was suggested by their crowding toward the light shortly after hatching in the incubator. The same tendency was indicated also by preliminary tests 4 and 5 (see table 1), in which no. 65 markedly preferred the brighter of two non-chromatic stimuli. When red and blue were approximately equal in brightness for the human eye, red was preferred by the chick. See preliminary tests 1–3. Even when red was considerably less bright than blue, as judged by the human standard, the chicks still selected red in preference to blue. After no. 65 had selected the dim red in series 6 in preference to the comparatively bright blue, series 8 and 9 were arranged to discover whether the animal was exhibiting either a negative reaction to blue or a positive reaction to darkness. The result showed clearly that neither of these possibilities obtained. Blue 10 was preferred to darkness, 9–1. But when the distance of the source light from the color screen was increased from 10 to 80 cm., the preference of the chick for blue as against darkness was only slightly evident. Again, when the stimuli were red 100 and blue 10, no. 58 and no. 65 each selected blue nine times, red once. When, with no. 58, blue was kept constant at this intensity and red was gradually increased in brightness to 40, the preference was as gradually shifted to red. A like result was found in the tests on no. 65 immediately following. Thus, while the chicks had a natural preference for the brighter of two non-chromatic stimuli, the same could not be said of chromatic stimuli as humanly estimated. The peculiar preference for red when opposed to blue of much greater brightness value pointed to

one or both of the following conclusions: (1) A difference exists between the chromatic luminosity values for the chick and those for the human; (2) chicks exhibit a qualitatively determined preference, such as is apparent in some other animals. Since, however, the preference of the chicks for red and blue varied with the variations in the relative brightness of the two color stimuli, brightness was clearly shown to be a controlling factor. Whatever natural preference the chicks might have had for red, this was overcome, in every case tried, by a sufficient relative increase in the intensity of the opposing blue. In view of these facts it was thought best at this stage of the investigation to seek such intensities of red and blue that initial preference for neither would appear. This point of indifference once located, it was planned to proceed with a process of training to determine the ability of the chicks to discriminate between two such red and blue stimuli. In the final preliminary tests red, from a source light 60 cm. distant from the screen, with a blue from a source of about equal power, 10 cm. distant, gave the reaction records of 6–4 and 7–3 for chicks no. 58 and no. 65. Red 70 with blue 10 resulted 4–5 and 3–7 for no. 61 and no. 65, respectively. Hereupon, the apparatus was slightly modified in preparation for the more careful preference tests that preceded training on the indifferent combination such as red 70, blue 10 seemed to approach. Cardboards containing circular openings 8 cm. in diameter replaced those with rectangular openings used in the preliminary tests. Circular water screens (Bausch and Lomb projection-lantern cooling cells) 12.5 cm. in diameter and 4.8 cm. thick were substituted for the rectangular cells in use up to this time. Furthermore, during the preference tests and throughout the following training and control tests, provision was made for the dark-adaptation of the animals by keeping them in the dark-basket in the dark-room for a period of ten minutes each day just prior to experimentation. Between trials also each chick was kept in the dark-basket. Among the preference tests, A–F, with red 70 opposed to blue 10, red was selected by the four chicks 28 times, blue 52 times, out of a total of 80 trials. Red and blue at these intensities, although not ideally balanced, were weighted on the proper side for a test of the chicks' ability to form a habit of positive reaction to red, so

training to this end was begun. The habit proved a comparatively easy one for the chicks to form, as evidenced by the first training series, 1–8. Each of the four animals finished the training, including the final twenty perfect reactions, within a total of 80 trials. When all the chicks had formed the red-blue habit perfectly, a slightly different intensity of blue was opposed to red at 10 and 100 alternately. The chicks continued the specific reaction to red, two of them without error. They had evidently not formed a habit of specific reaction to red or blue of a particular brightness value. As soon as the original habit was ascertained to be in perfect condition in all the chicks, they were tested further with red and blue, each color this time being used equally often at the value of 10 against the other color at 100 in a series of ten trials. Surprisingly, not a single error was made in this series by any chick. It is well to bear in mind that blue 10 was without question much brighter than red 100; and red 10 than blue 100.

Elsewhere[3] the writer has said something about Yerkes' discrimination method and the peculiar emphasis that sometimes attaches to the stimulus in connection with which the electric shock is administered. This emphasis on the blue was very noticeable in the results from all four chicks. When white light was opposed to blue in the control test, the chicks uniformly rejected blue, regardless of which stimulus had the greater brightness value. When white was substituted for blue and the factor of brightness eliminated by variation, the score was red 29, white 27, in a total of 56 trials. No. 61, however, with a little training formed a perfect red-white habit, the element of brightness being again eliminated in the manner just mentioned.

What shall be our interpretation of the above facts? When the stimuli were red 70 versus blue 10 in the preference tests, no marked preference was shown for either stimulus. Preference shifted decidedly to blue in the combination red 100 versus blue 10, and just as decidedly to red in the combination red 40 versus blue 10. That is, from the indifference point preference varied as the relative brightness of the stimuli varied. It seems reasonable to suppose, therefore, that at the above indifference values, the two stimuli were approximately equal in

[3] *Loc. cit.*, p. 69.

brightness for the chicks, provided brightness alone was the determining factor in the preference. The readiness, however, with which the chicks came to discriminate these two stimuli leads one to suspect that the basis of the discrimination was not difference in brightness. But this result alone is not proof conclusive that color was a factor. A point of brightness indifference may not mean brightness equality, nor need brightness equality for the chick in advance of training mean brightness equality after training. The selective reaction might therefore have been based on difference in brightness. But the evidence that color was a factor in these reactions does not rest on the fact alone that the indifference stimuli were discriminated. The fact that blue of different brightness value from that used in the training was rejected consistently when appearing with red that was not the same in brightness as the original red, ruled out the probability of specific reaction to specific brightness values. And more important still was control test 5. When red was increased in brightness from 70 to 10 and the accompanying blue decreased from 10 to 100, blue was rejected. When red was decreased from 70 to 100 and used with blue 10, blue was again rejected. Alternations of these combinations failed to interrupt the specific rejection of blue. It should be borne in mind that red 10 was brighter than blue 100 both for the chick (see preference test C) and the human; and red 100 was less bright than blue 10 both for the chick (see preference test F) and the human. Finally, in the control test on white-blue, the rejection of blue continued perfectly when white was much brighter and when it was much less bright than blue. Thus it seems almost certain that the quality of the stimulus as well as quantity played a determining part in the reactions of these animals.

An attempt was made to photometer the stimuli with a Lummer-Brodhun photometer and a Hefner amyl acetate lamp, but without success, primarily on account of the very low intensities of the weaker stimuli. But even had the intensities of the colored lights been sufficiently high for examination by this method, limited progress would have been made toward an objective measurement of the color values, for this method depends upon a human judgment such as we have already given. Radiometry is the hope of the experimenter in this difficulty.

TABLE 1

RED-BLUE REACTIONS

Chicks hatched Oct. 26, 1908

Series	Date	No. 58 Sex, F. Red	Blue	No. 61 Sex, M. Red	Blue	No. 62 Sex, M. Red	Blue	No. 65 Sex, M. Red	Blue	Remarks
					Preliminary Tests					
1	Nov. 17			10	0	10	0			Both lamps at 40
2	" 18			10	0	9	1			Both lamps at 40
3	" 19					10	0			Both lamps at 40
								Btr.	Dkr.	
4	Nov. 18							8	2	Lamps at 20 and 70, varied r
5	" 19							10	0	and l. Opal glass.
								Red	Blue	
6	Nov. 20	10	0	10	0			10	0	Right lamp, 10; left, 80.
7	" 20					10	0			Right lamp, 20; left, 70.
								Dk.	Blue	
8	Nov. 24							1	9	Darkness; B, 10.
9	" 24							4	6	Darkness; B, 80.
								Red	Blue	
10	Nov. 25	1	9					1	9	R, 100; B, 10.
11	" 25	0	6							R, 50–80; B, 10.
		3	1							R, 40; B, 10.
12	" 25							5	0	R, 40 and 50; B, 10.
								2	3	R, 60; B, 10.
13	" 26	6	4					7	3	R, 60; B, 10.
14	" 26			4	5			3	7	R, 70; B, 10.
					Preference Tests					
A	Nov. 28	6	4	1	9	3	7	4	6	R, 70; B, 10.
B	" 30	5	5	1	9	3	7	5	5	R, 70; B, 10.
C	Dec. 1	9	1	1	9	9	1	9	1	R, 40; B, 10.
D	" 1	0	10	0	10	0	10	5	5	R, 100; B, 10.
E	" 2	10	0	3	7	8	2	9	1	R, 40; B, 10.
F	" 2	3	7	1	9	1	9	5	5	R, 100; B, 10.
					Training Tests					
1	Dec. 4	5	5	4	6	5	5	5	5	R, 70; B, 10. Sk, 6
2	" 5	5	5	8	2	7	3	6	4	R, 70; B, 10. Sk, 6.
3	" 7	9	1	7	3	6	4	7	3	R, 70; B, 10. Sk, 6.
4	" 8	9	1	8	2	6	4	9	1	R, 70; B, 10. Sk, 6.
5	" 9	10	0	8	2	10	0	10	0	R, 70; B, 10. Sk. 6.
6	" 10	10	0	8	2	8	2	10	0	R, 70; B, 10. Sk, 6.
7	" 11			10	0	10	0			R, 70; B, 10. Sk, 6.
8	" 12			10	0	10	0			R, 70; B, 10. Sk, 6.
					Control Tests					
1	Dec. 13	10	0	7	3	8	2	10	0	R, 10 & 100 alt.; B, 20.
2	" 14	9	1	10	0	10	0	10	0	R, 70; B, 10. Sk, 6.
3	" 14	9	1							R, 70 B, 10. Sk, 6.
4	" 14	10	0							R, 70; B, 10. Sk, 6.
5	" 15	10	0	10	0	10	0	10	0	R & B, 10 & 100, alternately.
6	" 16	8	2					9	0	R, 70; B, 10.
				8	0	6	4			R, 7–100; B, 7–105.
7	" 16			1						R, 100; Darkness.
					1					Darkness; B, 10.
8	" 17	6	0	6	0	5	1	6	0	R, 70; B, 10.
		White	Blue	White	Blue	White	Blue	White	Blue	
9	Dec. 17	10	0	10	0	10	0	10	0	B=, <. and > W in brightness
		Btr.	Dkr.	Btr.	Dkr.	Btr.	Dkr.	Btr.	Dkr.	
10	Dec. 18	2	6	8	0	6	2	5	3	Both sides W. Opal glass.
		Red	Blue	Red	Blue	Red	Blue	Red	Blue	
11	Dec. 21	4	0	4	0	4	0	4	0	R, 70; B, 10.

TABLE 1—*Continued*

Series	Date	No. 58 Sex. F. Red	White	No. 61 Sex. M. Red	White	No. 62 Sex. M. Red	White	No. 65 Sex. M. Red	White	Remarks
						Control Tests—Continued				
12	Dec. 21	8	6	10	4	4	10	7	7	R. btr. 6 trials; W. btr. 6; eq. 2.
		Red	Blue	Red	Blue	Red	Blue	Red	Blue	
13	Dec. 21	5	1	4	2	4	2	4	2	R and B varied in brightness, now one, now the other brighter, with extreme diff's.
		Red	Blue	Red	Blue	Red	Blue	Red	Blue	
14	Jan. 4	4	0	4	0	4	0	4	0	R. 70; B. 30.
		Red	White	Red	White	Red	White	Red	White	
15	Jan. 4	5	5	8	2	6	4	5	5	R. 6–30; W. 15–105.
						Training Tests				
1	Jan. 5			9	1	5	5			R. 6–30; W. 15–105
2	" 6			8	2	9	1			R. 6–30; W. 15–105.
3	" 6			9	1	8	2			R. 6–30; W. 15–105.
4	" 7			10	0	9	1			R. 6–30; W. 15–105.
5	" 7			10	0	8	2			R. 6–70; W. 70–105.*
6	" 8					6	4			R. 6–70; W. 70–105.
7	" 8					7	3			R. 6–70; W. 70–105.
8	" 8					7	3			R. 6–70; W. 70–105.
9	" 9					8	2			R. 6–70; W. 70–105.
										Note: No. 62 large, slow and inactive from start. At time of last test physically weak.

* R. 10 > W. 100 in brightness.

Explanation of abbreviations in table: R, red; B, blue; W, white; Sk, electric shock; Dk, darkness; Btr., brighter; Dkr., darker; Eq., equal.

A numerical value appearing with the name of the stimulus represents distance in centimeters of the source light from the screen.

REACTIONS TO FORMS

As a test of form discrimination chicks were afforded an opportunity to select a circle when appearing along with a square. For this experiment the sliding frame of the apparatus was fitted with three black tin plates, in two of which had been cut square openings each with a side of 4.43 cm.; in the third and central one was cut a circular opening of the same area, its diameter being 5 cm. Compactly behind each of the form plates were mounted two plates of opal glass, each pair of plates having an aggregate thickness of about .515 cm. The lamps were stationed at a distance of 90 cm. from the screen. The Bausch and Lomb projection-lantern water cells were adjusted immediately back of the opal glass plates, on the side of the source lights. With the above arrangement white lighted areas with sharp outlines were presented to the animals, with variations in brightness, size, and position under good control.

Three chicks, nos. 73, 76, and 87 were used in these tests, one of which yielded positive results. After 60 training tests,

no. 73 was unfitted for further work by a misuse of the electric shock. When no. 87 had completed 160 trials in the training, the work was discontinued on account of the poor physical condition of the animal. Starting with preference trials of 9 reactions to the circle and 11 to the square, its total number of reactions to the circle during the training was 101; to the square, 59. The final two series registered 9–1 and 8–2 in favor of the circle. There were thus indications that the training had been somewhat effective.

The results from no. 76, on the other hand, were more convincing. Reacting indifferently in the preference tests to the circle and the square of equal area and brightness, it was able to select the circle 38 times out of the last 40 trials in a training series of 120 trials. When the positions of the source lights were shifted from 90 to fixed positions at 80 and 100 cm. respectively, so that the forms were illuminated unequally, no. 76 selected the circle 18 times out of 20 trials. Training was then resumed for two series with the forms of equal brightness, after which the sliding screen was mounted from left to right as follows: 5 cm. circle, 4.43 cm. square, and a circle equal to that circumscribed about the 4.43 cm. square. Ten trials with this combination netted 8 selections of the circles and 2 of the square. When, however, the next change in the forms was made, there appeared to be no sign of discrimination. The corresponding inscribed circle was substituted for the circumscribed, accompanying as in the previous test the square and circle equal to each other in area, and the first ten reactions recorded were 5 for the circles and 5 for the square. An attempt was now made to train the chick to select these circles in preference to the square. After this training had been prolonged for 70 trials, the chick was selecting the circles as against the square 9–1. At this point the frame was set with the circumscribed and the inscribed circles in combination with the square, whereupon the chick made the record 10–0. For the series following this one, conditions remained the same except that the lamps were exchanged in the illumination boxes and a different tin plate was used to produce the square form. The series resulted 9–1. When this series was repeated with a different order of shifts of the frame, the animal made the record 10–0. For data see table 2.

TABLE 2

CIRCLE-SQUARE REACTIONS

Chick No. 76. Sex, F.

Hatched December 3, 1908

Series	Date	Right	Wrong	Remarks
A	Jan. 21	4	6	Cir. and Sq. equal in area and brightness.
B	" 22	5	5	" " "
1	" 23	6	4	" " "
2	" 24	6	4	" " "
3	" 25	2	8	" " "
4	" 25	7	3	" " "
5	" 25	10	0	" " "
6	" 26	6	4	" " "
7	" 26	9	1	" " "
8	" 26	5	5	" " "
9	" 26	9	1	" " "
10	" 26	10	0	" " "
11	" 26	10	0	" " "
12	" 26	9	1	" " "
		Control Tests		
1	Jan. 27	9	1	Lamp, r, 80; lamp, l, 100.
2	" 27	9	1	Lamp, r, 100; lamp, l, 80.
		Training Tests		
13	Jan. 30	7	3	Cir. and Sq. equal in area and brightness.
14	" 30	9	1	" " "
		Control Tests		
3	Jan. 30	8	2	Sq. vs. equivalent and circumscribed circles.
4	" 30	5	5	Sq. vs. equivalent and inscribed circles.
		Training Tests		
15	Jan. 30	8	2	Sq. vs. equivalent and inscribed circles.
16	" 30	6	4	" "
17	" 30	7	3	" "
18	" 30	8	2	" "
19	" 30	9	1	" "
20	" 31	7	3	" "
21	" 31	9	1	" "
		Control Tests		
5	Jan. 31	10	0	Sq. vs. circumscribed and inscribed circles.
6	" 31	9	1	Same, lamps exchanged. New Sq. form plate.
7	" 31	10	0	Same, different order of stimuli.

After negative results in several experiments on forms, the results obtained with no. 76 are interesting. I believe I was fortunate in the choice of animal for this work. My notes

describe this chick as rather undersized, strong, active, sensitive, cautious, and quiet. Detailed records of its reactions show that selection of stimuli was made by affirmation and negation most frequently, by comparison occasionally.

REACTIONS TO SIZES

The method of testing size discrimination was similar to that followed in the tests on forms. Two circular areas of different size were presented to the chicks, one 5 cm. in diameter, the other 8 cm. In the sliding frame of the apparatus an 8 cm. circle was mounted at each end, a 5 cm. circle in the middle. In other respects the appointments of the apparatus were the same as in the form experiments.

In these tests three factors were taken into consideration, any one of which might have become a means of discrimination:

1. Difference in size of the lighted areas,
2. Difference in brightness of the lighted areas, and
3. Difference in brightness of the right and left sides of the experiment box.

To eliminate other means of discrimination but the difference in size of the lighted surfaces, the following list of tests was adhered to throughout the training:

1. Sides of box equal in brightness:	S, 60, r;	L, 96, l
2. Sides of box equal in brightness:	S, 60, l;	L, 96, r
3. Circles equal in brightness:	S, 96, l;	L, 96, r
4. Circles equal in brightness:	S, 96, r;	L, 96, l
5. Large circle the brighter:	S, 80, l;	L, 40, r
6. Small circle the brighter:	S, 40, r;	L, 80, l
7. Large circle much the brighter:	S, 100, l;	L, 30, r
8. Small circle much the brighter:	S, 30, r;	L, 100, l
9. Sides of box equal in brightness:	S, 60, r;	L, 96, l
10. Circles equal in brightness:	S, 96, l;	L, 96, r

Explanation of abbreviations: S, small circle; L, large circle; numbers, distances in centimeters of source lights from screen; r, right; l, left.

The experiment was undertaken with chicks no. 70 and no. 83. The preference tests showed a marked inclination of the chicks to react positively to the larger or negatively to the smaller of the two circles, each animal reacting 8 times to L

and only 2 to S. The plan of the work was interfered with somewhat by the necessary retirement of no. 70. After 120 trials this animal was unable to continue on account of physical debility, an unfortunate failing of these laboratory chicks after a given period. No. 70, however, showed signs of acquiring the small-large habit, for in its last 50 trials there were 38 reactions to S and 12 to L. No. 83 remained in the experiment with the positive result shown in table 3, which follows:

TABLE 3

SMALL-LARGE REACTIONS

Chick No. 83. Sex, M.

Hatched December 12, 1908

Series	Date	Right	Wrong
A	Jan. 13	2	8
1	" 14	2	8
2	" 15	3	7
3	" 16	5	5
4	" 18	5	5
5	" 19	6	4
6	" 20	3	7
7	" 21	7	3
8	" 22	8	2
9	" 23	5	4
10	" 24	5	5
11	" 25	6	4
12	" 27	4	6
13	" 27	7	3
14	" 27	5	5
15	" 27	6	4
16	" 27	9	1
17	" 27	6	4
18	" 28	8	2
19	" 28	8	2
20	" 28	9	1
21	" 28	8	2
22	" 28	10	0
23	" 28	10	0
24	" 28	10	0

SUMMARY AND CONCLUSIONS

The chicks in advance of training selected the brighter of two non-chromatic stimuli.

According to the human standard of brightness values, a similar law did not hold for chromatic stimuli. This exception points in the direction of one or both of the following conclusions:

(1) A difference exists between the chromatic luminosity values for the chick and those for the human;

(2) Chicks exhibit a qualitatively determined preference, such as is apparent in some other animals.

Brightness was found to be a controlling factor in the responses to optical stimuli.

All four chicks after brief training formed the habit of selecting without error one of two colors at the brightness-indifference point.

This selective reaction was not determined by red or blue of particular brightness values; neither was it determined by the relative brightness of the two stimuli.

Thus seemed verified a conclusion of our earlier work, namely, that quality as well as quantity of the optical stimulus played a determining part in the reactions of these animals.

Positive reaction to a given stimulus as indicated in a table of reactions did not necessarily imply specific reaction to that stimulus. It sometimes meant merely a specific rejection of the opposing stimulus.

A chick learned to discriminate two optical stimuli on the basis of difference in form.

A chick learned to discriminate two optical stimuli on the basis of difference in size.

I

No. 11.

BEHAVIOR MONOGRAPHS

Volume 1, Number 1, 1911 Serial Number 1

Edited by JOHN B. WATSON
The Johns Hopkins University

The Development of Certain Instincts and Habits in Chicks

FREDERICK S. BREED

Assistant Professor of Education
The University of Michigan

From the Harvard Psychological Laboratory

Published
at Cambridge Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E. STECHERT & CO., London, Paris and Leipzig, Foreign Agents

III

CONTENTS

PART I. INSTINCTIVE REACTIONS

I. Introduction and Statement of the Problem

The experimental work reported in this monograph was carried on during the years 1907, 1908, and 1909 in the Harvard Psychological Laboratory. The variety of chick used in all the work was the Barred Plymouth Rock. It is usually considered by poultry breeders a hardy chick, and for this reason more than any other was selected for these tests. The program of research began with a cursory study of the first activities of the animals, preliminary to a more detailed study of certain instinctive modes of response such as drinking, pecking, and "imitating." In the case of drinking, interest centered in the nature of the stimulus; in the case of pecking, in the accuracy of the reaction; as for imitation, the question was asked, In how far is social influence a means to improvement in the accuracy of pecking? After some knowledge of the natural tendencies of the chicks had been gained, an attempt was made to trace the course of development of certain habits of response to optical stimuli, as well as to study the interrelation and persistence of these habits. Quantitative methods were devised and applied wherever they suggested themselves to the experimenter.

The investigation was pursued under the immediate direction of Professor Robert M. Yerkes. What I owe to his searching criticism and fertile suggestions, a mere word of acknowledgment will not suffice to say. I shall be only too glad if my immature effort reveal some trace of his admirable scientific spirit. I am also much indebted to my colleague, Professor Charles Scott Berry, for a critical reading of the manuscript, and to Dr. William F. Hauhart, Instructor in German, University of Michigan, for valuable assistance in reading the proof.

II. Previous Experimental Work on Chicks

Attention is limited in this résumé to the conclusions of investigators in regard to the accuracy of the pecking response. The results of some previous studies of the drinking reaction will be

discussed in connection with the results of a study of the reaction reported later in the paper.

Spalding's [1] observations on the pecking reaction have been quoted widely. Concerning the pecking of his chicks he says: "They did not attempt to seize things beyond their reach, as babies are said to grasp at the moon; and they may be said to have *invariably hit the objects at which they struck*—they never missed by more than a hair's breadth, and that too, when the specks at which they aimed were no bigger, and less visible, than the smallest dot of an *i*. To seize between the points of the mandibles at the very instant of striking seemed a more difficult operation. I have seen a chicken seize and swallow an insect at the first attempt; most frequently, however, they struck five or six times, lifting once or twice before they succeeded in swallowing their first food." (*Italics mine.*)

On the question of the accuracy of the pecking of chicks, Preyer [2] disagrees with Spalding. "I cannot admit," he says, "the supposed infallibility to within a hair's breadth. They miss in pecking by as much as two millimeters, though seldom. On the other hand, the attempts at swallowing frequently fail. Here it should be considered that even grown fowls are not sure in their pecking, seizing, and swallowing, as any one that observes closely may easily perceive. *The accuracy is, however, marvelous at the very beginning.*" (*Italics mine.*)

Romanes,[3] though reporting no experimental work of his own, deserves notice on account of his wide influence. He quotes Spalding extensively and with approval. For Romanes, perfection as applied to instinct means perfect adaptation independent of individual experience. He illustrates such perfection "by considering *the wonderful accuracy of many among the highly refined and complex adjustments* which are manifested by the newly-born young of the higher animals," (italics mine), citing first in his list of examples of perfection the pecking reaction of chicks as reported by Spalding.

Eimer [4] experimented on chicks during two different years. The impression he got of the accuracy of pecking appears in the

[1] Spalding, D. A.: Instinct. With original observations on young animals. *Macmillan's Mag.*, 1873, vol. 27, p. 284.

[2] Preyer, W.: The senses and the will. (1881.) Tr., New York, 1890, p. 67.

[3] Romanes, G. J.: Mental evolution in animals. New York, 1884, p. 161.

[4] Eimer, G. H. T.: Organic evolution. Tr., London, 1890, p. 246.

following report of the reactions of one of his animals: "At the first attempt it touched a grain with as much certainty as if it had pecked at millet for ever so long. But it did not succeed in taking up the grain in its beak. A second attempt immediately afterwards likewise failed, but at the third, which succeeded without a pause, the grain was grasped and swallowed, and now the little creature *went on feeding as though it had done so for years!*" (Italics mine.)

Mills [1] thinks that some of Spalding's statements exaggerate. "Instinct is not the hard and fast thing it is sometimes supposed to be." On the basis of his own experiments he concludes: "*Thus one* (chick) *may strike a crumb accurately every time it pecks*, and pick it up on the first attempt; another misses, or shows great difficulty in getting it into the mouth." (Italics mine.)

Morgan [2] concludes that the complicated process of striking, seizing, and swallowing "is performed so soon, and with so few trials—often at the third or fourth attempt—that *one must regard the whole as essentially congenital in its definiteness*, and look upon the few preparatory efforts as merely the steadying of the inherited organic apparatus to its work." (Italics mine.) By congenital response is meant one that is not the result of acquired skill.

Writing earlier on the same topic, Morgan [3] had said the chicks "required a little intelligence, acting by and through experience, to perfect their (instinctive) activities. The instincts were very nearly, but not quite, perfect."

Thorndike [4] criticises Morgan's conclusions on the improvement in accuracy of the pecking reaction. "Lloyd Morgan, for instance, has chosen a dubious example of perfecting through habit in the seizing of bits of food by chicks. They often do fail to seize in their first experiences, as he observed, *but they often, perhaps just as often, fail even after long experience.*" (Italics mine.)

Finally, a further matter, aside from the question of the

[1] Mills, W.: The psychic development of young animals and its physical correlation. Trans. R. S. Can., Sec. IV, 1895; or, The nature and development of animal intelligence, New York, 1898, p. 262.

[2] Morgan, C. L.: Habit and instinct. London and New York, 1896, p. 37.

[3] Morgan, C. L.: Introduction to comparative psychology. London, 1894, p. 207.

[4] Thorndike, E. L.: The instinctive reaction of young chicks. *Psych. Rev.*, VI, 1899, p. 282. Also, Instinct. *Biol. Lectures*, Woods Hole, 1899, p. 61.

accuracy of pecking. Katz and Révész [1] support Hess [2] in the view that the hungry chick does not peck at food which it does not see. The former assert, for example, that a chicken in the dark-room, even if placed directly in the food, will not peck at it. The point happens to be vital to their method in some important work.

It has seemed best to report the essential conclusions of the writers mentioned, so far as possible, in their own words. It may not be amiss, however, to note two general impressions one gets from these studies of the pecking instinct: (1) that the reaction has a high degree of congenital accuracy, and (2) that it has less of this than was formerly supposed.

III. General Apparatus

The room used for these experiments had a western exposure. It was equipped with an incubator [3] and a brooder [4] so that chicks for the experiments might be hatched at any time during the year in the laboratory. Such an arrangement also provided a good opportunity for complete knowledge of the history of the animals. For the sake of safety, economy, and convenience, both brooder and incubator were heated with gas. In addition to the regular thermostat on the incubator, a gas regulator [5] was attached to insure a more constant temperature. No trouble was experienced at any time in securing eggs of the special kind desired. The incubator and brooder were easily operated after a careful study of the directions that came with the machines. The percentage of fertile eggs hatched during the first year was 65.6. At times two settings were run simultaneously in the same incubator.

---

[1] Katz, D. und Révész, G.: Experimentell-psychologische Untersuchungen mit Hühnern. *Zeitschrift f. Psych. u. Physiol. d. Sinnesorgane*, 1909, Bd. 50, S. 93.

[2] Hess, C.: Ueber Dunkeladaptation und Sehpurpur bei Hühnern und Tauben. *Archiv f. Augenheilkunde*, 1907, Bd. 57, S. 298–316.

————: Untersuchungen über Lichtsinn und Farbensinn der Tagvögel. *Ibid.*, 1907, Bd. 57, S. 317–327.

————: Untersuchungen über die Ausdehnung des pupillomotorisch wirksamen Bezirkes der Netzhaut und über die pupillomotorischen Aufnahmeorgane. *Ibid.*, 1908, Bd. 58, S. 182.

————: Untersuchungen über das Sehen und über die Pupillenreaktion von Tag-und von Nachtvögeln. *Ibid.*, 1908, Bd. 59, S. 143–167.

[3] Junior No. 2, sold by the Prairie State Incubator Co., Homer City, Pa.

[4] Indoor Brooder, No. 5, sold by the same firm.

[5] Single Float Standard Automatic Gas Regulator, secured from the Gas Consumers' Association, 18 Boylston St., Boston, Mass.

Greater freedom of movement was provided for the birds by the use of two cages that were connected with the brooder. For litter, white sand and shredded alfalfa were found satisfactory. Sand is more suitable than ordinary earth where it is desirable to avoid dust. A chick food [1] that is regularly on the market served our purposes very well. With the above simple equipment the chicks for the experiments were hatched and kept in the animal wing of the laboratory.

IV. Observations and Experiments

A. First Activities

If one would have the ontogenetic history of a chick, he must begin his study of the development of activity while the chick is still within the egg. A movement of the egg tray was sometimes found to be the occasion of a chirp within the egg hours before the shell was chipped. Or, chirping at times occurred so loud that it could be heard distinctly in the room in which the incubator was situated, when the incubator was tightly closed and no external stimuli, such as noises and jars, were noticed. Before the shell was broken at any point, a continuous tapping sound could often be heard within the egg, somewhat as if the chick were pecking against the inside of the shell. The movements of chicks just after the shell was chipped were observed through the glass door of the incubator and also seen to good advantage when the eggs were removed from the incubator and put in an appropriate receptacle under an electric light. Upon these latter occasions the ragged edges of the egg shell near the beak of the chicken were removed to give a better view of the animal's movements. Upon the observations following the transfer of a chipped egg to a nest warmed by an electric light, I find the following note: "I broke away much of the shell. After bill of chick was entirely out of reach of the shell, the chick continued to make a sound somewhat like pecking, and that, too, without opening and closing the bill. The 'clicking' sounds seemed to accompany the rhythmical heaving of the animal, which I took to be breathing. . . . Eyes were open, apparently, as soon as chick had unfolded itself." At the next hatch another chick was observed under an electric

[1] Cyphers Chick Food, sold by the Cyphers Incubator Co., Boston, Mass.

light in the same way. I follow my notes: "Made at times a regular clicking noise with breathing. Frequently clapped mandibles together. . . . Repeatedly raised bill with a lifting motion of the head. This reaction might bring tip of bill, and hence the bill scale, in contact with the shell. Opened mouth and lifted bill. Eyes closed while in egg. Sometimes pushed bill forward, following this movement with a clapping together of the mandibles."

While the chicks were still in the egg the legs were folded well up on the breast and the head was turned down toward the breast. After the egg was chipped so that the behavior of the animal could be observed, the lifting movement of head and beak above referred to was more frequent than any other. One much less often saw anything like a forward thrust of the bill.

The chicks came out of the eggs wet and, in a temperature of 103° F., remained several hours before drying off. Spells of vigorous activity alternated with periods of passivity during the chicks' struggles just prior to hatching, as well as for some hours after hatching. The post-embryonic life of the chick in its early hours seemingly prolonged without interruption the life in the embryo. Chicks appeared to break the shell in two by a lifting, struggling movement of the head accompanied by a stretching, straightening movement of the legs. At least I have observed this combination of movements at the moment of hatching. Now if one watch carefully the behavior of a chick for some time after exclusion, he finds that there are these similarities between the later life within the shell and the earlier life outside the shell. The positions of the head and legs within the shell correspond to the positions assumed by these members during the passive states shortly after hatching. In both cases activity and passivity alternate, the periodicity of which alternation seems to be largely determined by intra-organic stimuli. If it be true that the legs participate in the action that finally breaks the shell, as I think they do, the lifting of the head and the pushing with the legs would be represented in the early post-embryonic life by those pulses of activity in which the chick lifts its head, rises to its feet, staggers a few steps, or struggles a few moments, and lapses again into the passive state. And just as the chick in the shell often claps its mandibles

together without marked activity in other regions of the organism, so the chick during the first few hours after exclusion may be seen to work its mandibles in a similar way without disturbing its sitting posture. The pulses of activity, embryonic and post-embryonic alike, are often accompanied by loud chirping.

One might expect to find more unmistakable evidence of a pecking reaction while the chick is yet within the egg. There is an impression abroad that the chick pecks its way out of the shell. When the position of the head in the embryo is taken into consideration, one can see two reasons why most of the movements of the head are lifting movements, and not pecking reactions: (1) the lifting movement tends to free the head and neck from their folded position, and (2) pecking would seem to be a difficult matter with the head folded down on the breast in this wise. Besides, the lifting reaction does actually break shell and tear confining membranes, and so is effective in releasing the chick. It may be found that the chick does not peck its way out of the shell. However, the matter will need more careful study. For hours I have watched chicks laboring in the egg to discover a clear case of the pecking reaction. At times, before the egg has been broken in two, one does see short, quick, forward thrusts of the bill followed by working of the mandibles. And chicks only a few hours out of the egg may be observed repeatedly executing what might be called a pecking reaction "into the air," followed by clapping together of the mandibles. The following note describes the reaction more adequately: "Chicks after exclusion, on becoming aroused from a dormant period, often open bill with a chewing motion and sometimes thrust the bill forward sharply into the air even without fully opening the eyes, no object, apparently, being pecked at. Noticed a number of times."

The following activities, generally recognized as instinctive, were observed within the incubator on the first day, before the chicks were completely dry: Preening the down of the neck, wings, and breast; flapping of wings; chirping; walking; pecking; lying on side and stretching out legs (in the rays of a 16 c.p. electric lamp); following a moving object with a motion of the head; and chirring. Scratching, twittering, and wiping of the bill have been noticed on the second day when the chicks were taken out of the incubator for the first time and placed on

a black cardboard for their pecking tests. Our knowledge of the time of appearance of the various instincts should not be left to depend entirely upon chance stimuli. Who shall say that a given reaction might not have occurred much earlier if the appropriate stimulus had been provided?

The chicks, while still in the incubator, are known to be positively phototropic. The incubator trays that have near the glass door a trap through which the chicks fall to a screen below, depend for their effectiveness on the fact that the chicks crowd toward the light.

B. Drinking

a. Problem.—In the study of instincts from the objective point of view, interest naturally centers first in function, second in structure. The activities which are known as instinctive must be analyzed into component units of behavior, of which they are nearly always complexes. Furthermore, no account of instinct will be satisfactory, no explanation complete, until we understand the structure of the machinery involved in each action. But so much accomplished, this is not all. These structures are not of such a nature that they in some way get themselves into action. So far as we know, they have no inherent principle of spontaneity. Intra- or extra-organic stimuli are necessary to touch them off. Environment in the form of energies external to the structures and additional to the functions seems to be a *sine qua non*. Hence a complete understanding of instinctive actions will include a detailed knowledge of the "objects" in conjunction with which the particular activities manifest themselves. In the following bit of work on the instinct of drinking, consideration of the problems of function and structure is made secondary to an inquiry into the nature of the extra-organic stimulus.

b. Method and Tests.—Chicks no. 69 to no. 81, inclusive, were hatched during the afternoon and evening of Dec. 2 and the morning of Dec. 3, 1908. Beginning with Dec. 3, their pecking had been tested in the regular manner each day. As the animals finished the first pecking tests they were marked, numbered, and transferred from the incubator to the brooder. Although given food and freedom to run about in the litter, they were allowed nothing at all to drink until tested as described below, neither were they permitted to see other chicks drink. A pos-

sible chance of coming in contact with something like water was afforded through deposits of waterish excrement. Even if this as a stimulus were able to elicit the drinking reaction, the fact that the chicks in the incubator rested on an elevated wire screen lessened the probability of the occurrence of such a stimulus. In the brooder the floor was constantly covered with the regular litter from one to two inches deep. On Dec. 5, from 3 to 5:30 p. m., when the chicks varied from 2.5 to 3 days of age, they were brought one at a time to the experiment table, just as if they were to be given a pecking test, and were returned to the brooder each as its drinking test was completed. In front of each animal was set a clean watch glass containing fresh water devoid at the start of bubbles or sediment. The watch glass rested on a square piece of smooth, white, plain note paper. As the observations on each chick were completed, the soiled paper was replaced by a clean piece, the dish was washed, the water renewed, and bits of food or drops of water carefully brushed from the table. The report of the observations follows in the order in which the chicks were tested.

While no. 70 was eating on the table, the watch glass, in the manner set forth above, was presented. The chick pecked grains which had been scattered over the white paper and then ran its bill in a forward direction along the paper in the drinking reaction. This it did repeatedly. Later I helped it to find the water in the dish. It seems unnecessary to state that precaution was taken to see that the paper was dry.

No. 72 was tried next. It pecked the plain, clean, white paper. Before the water was found by no. 72, no. 70 was brought out and allowed to drink in the presence of no. 72. No. 72 followed no. 70 about, performing the drinking reaction along the edge of the dish. One of the chicks stepped into the dish and carried some water to the paper surrounding it. No. 72 got its bill into this and forthwith responded with the drinking reaction. Then it wandered about the table and ran its bill along the black leather of my watch fob, 30 cm. distant from the dish, giving the drinking reaction. This does not mean that it touched the fob with its bill and then lifted its head in the manner so well known. The reaction to the fob was just what one sees when he watches a chick gathering water into its bill and throat while the bill is inserted in the liquid. This reaction is

not easily confused with pecking. It was repeated on the silver charm of the fob.

No. 71 learned to drink by pecking at a bit of excrement that chanced to get into the water. Time after time it pecked about the edge of the dish. It was not observed to dip its bill into clear water. It secured water first by pecking and persisted in getting it this way throughout these first trials.

No. 74, when set on the table, pecked the edge of the white paper. First contact of the bill with water came from pecking a drop of water deposited on the paper. I put its bill into the water in the dish, whereupon it reacted to the situation by pecking the edge of the dish.

No. 77. By catching the paper on which the dish rested, the water was caused to wave slightly in the presence of the chick. Its head went down hard in a pecking reaction and hit the bottom of the dish, following which the head was lifted in the manner characteristic of drinking. On the second trial the head approached the water not in the manner of pecking but of drinking. The pecking approach to an object is decidedly different from the drinking approach. In contrast to the sharp descent upon the object in the former, the low gentle reaching movement in drinking, accompanied by a straightening of the neck, opening of the mouth, and a peculiar motion of the throat, is most marked.

No. 69. The surface of the water was agitated as for no. 77. No. 69 pecked directly into the water. Shortly afterward it pecked the water without my shaking it and at a point where I could discern no special stimulus like a bubble or particle of food. The positions of chick and dish were such that light reflected from the water might have been a factor.

No. 75. Pecked edge of the white paper. Pecked about the edge of the dish and its bill slipped into the water. The chick then pecked the edge of the dish twelve times, the bill getting into the water during some of these reactions. Pecked twice into the dish and got a little water. Of the next twenty reactions all were pecks and all but two were directed toward the side and edge of the dish.

No. 80. I thought I observed the chick performing the drinking reaction on a piece of glazed kymograph record paper about 3 cm. square that I vibrated between two fingers in front of

the chick. But the reaction came out unmistakably a moment later on the side of the glass dish. The water was still, the glass was dry, and the chick ran its bill along the edge of the glass exhibiting plainly the drinking movements without touching the water. It later found the water and began to drink without assistance.

No. 81. On the first attempt it dipped its bill into the still water and drank from a point at which I could distinguish no special object in the water. While crouching by the dish drinking, as its bill was coming down slowly at the termination of a drinking reaction, it turned its head to the left, touched its bill gently to a bit of dry chick food lying within reach, and performed the drinking reaction upon it. It did not eat the grain.

No. 79. Pecked side of dish. Ran beak along edge of dish. Its beak (accidentally) slipped into the water. After this the chick began to drink. Frequently ran beak down the outside of the dish.

No. 73. Did not peck at dish nor find water. No. 74 was placed on the table with no. 73. No. 74 pecked five times at the edge of the dish, not touching the water, when no. 73 began to peck near the same place. The latter's bill slipped into the water and it began to drink energetically.

No. 76. I shook the water in its presence and it pecked into it. It began to drink and twitter. Pecked the edge of the dish. Ran its bill along the outside and inner edge of it.

c. Discussion of results.—Spalding,[1] commenting on the drinking of chicks, remarks: "It also appeared that, though thirsty, they did not recognize water by sight, ... and they had to some extent to learn to drink." Discussing the same instinct, Morgan [2] says: "The statement of fact (so far as my observations go) that I made was this: That the sight of still water evoked no instinctive response; but that the touch of water in the bill at once evoked the characteristic instinctive behavior." Mills [3] expresses his opinion thus: "It is not primarily so much the sight, but rather the touch of water . . . that in the very first instance leads to drinking."

[1] Spalding, D. A.: *Loc. cit.*, p. 288.

[2] Morgan, C. L.: The habit of drinking in young birds. *Science*, N. S., 1896, vol. 3, p. 900.

[3] Mills, W.: The nature and development of animal intelligence. New York, 1898, p. 281.

On the basis of these and similar observations, it has been asserted that a chick swallows water instinctively, but must learn to drink by imitation or accident; that is to say, the drinking instinct requires supplementation. A passage from Baldwin [1] will illustrate: "In the case of the fowl's drinking, it is not the mere fact that drinking and eating may differ in the degree to which the performance is congenital; the reports seem to show that this varies in different fowl; but that instincts (in this case drinking) may be only half congenital, and may have to be supplemented by imitation, accident, intelligence, instruction, etc., in order to act, even when the actions are so necessary to life that the creature would certainly die if the function were not performed. That is the interesting point."

For the sake of clearness in the discussion of drinking, the parts of the drinking complex must be more sharply distinguished. There is (1) the approach to the object, elicited evidently by optical stimulation, (2) a sort of rhythmic movement of mandibles and throat, which brings the object within the mouth, and (3) the swallowing activity, which is evoked by stimuli resulting from the contact of water with the mouth, and which is marked by an elevating movement of the head and neck. We find an exactly parallel series upon analysis of the pecking reaction: (1) striking, (2) seizing, and (3) swallowing. Any full account of the drinking instinct must include the approach to the object as well as the subsequent manipulation.

No one, I think, will deny that the touch of water in the bill evokes reaction 3 of the above series. And we know that chicks may get this appropriate contact-stimulus indirectly by pecking. Furthermore, the drinking of one chick in the presence of another often stimulates this other to become active about the water and thereby leads to its drinking. That is, drinking usually does begin as the result of a contact stimulation mediated by the prior activity of the pecking and imitating instincts. But this at once suggests the further question, Are imitating and feeding necessary precursors to reaction 1 of the series, the movement of approach? The results of the foregoing experiments seem to show clearly that the drinking instinct is self-dependent in so far as its relation to these other instincts is

[1] Baldwin, J. M.: Instinct. *Science*, N. S., 1896, vol. 3, p. 669.

concerned. When chicks, without having previously drunk, respond with the drinking reaction to the surface of smooth white note paper, the edge of white glazed kymograph paper, or the edge of a glass dish, all these objects must be supposed to have some quality or qualities, undoubtedly visual, which evoke the drinking reaction. Here are stimuli that call forth a first drinking reaction independently of other instincts. If further evidence were needed to substantiate the conclusion that the instinct is self-dependent, it is found in certain other observations. After the first actual drinking, the drinking response was made to a grain of food, a piece of black leather, a silver ornament, and, in the case of chicks other than those studied in the special experiment, to a line in some dust on a smooth surface, a white spot on the experiment table, the clean surface of black cardboard, and the polished surface of a table.

Thus the feeding and drinking instincts are more similar than writers have hitherto supposed. In the case of feeding, the fact seems to be that newly hatched chicks respond to a great variety of objects indifferently, and only later come to select those which are food from the rest; in the case of drinking, the observations show that, if the need be sufficiently urgent, a large variety of objects in like manner elicit the action, and apparently with a like result. The indiscriminate use of reaction 1 of the drinking instinct may bring the appropriate stimulus for reaction 3, without the co-operation of the pecking and imitative activities. The drinking instinct, therefore, does not "have to be supplemented by imitation, accident, intelligence, instruction, etc., in order to act."

Finally, is still water a sufficient stimulus for the act of drinking? I am by no means ready to say it is not. I regret that my experiments are not so complete on this point as they might have been. There is general agreement at present that chicks do not begin to drink in response to this optical stimulus. But deprive them of water for a sufficient length of time after hatching and perhaps the "sight of still water" will evoke this instinctive reaction. It is not improbable that the effective element or elements in the objects which have been observed to draw forth the reaction are common also to water.

C. Pecking

a. Apparatus and method.—A detailed and prolonged study of the pecking reaction was now attempted. The literature on this topic reveals the fact that the interest in the accuracy of this instinctive activity has been the central one. As suggested before, the accuracy of the reaction became the central interest also in the investigation that is reported in the following pages.

The apparatus used in these experiments was very simple. A table with a hard polished surface was set near a window where there was good light. To this table the chicks were each morning brought, one at a time, and permitted to eat in a natural way from the surface of a piece of black cardboard about 20 cm. wide and 25 cm. long. Carried daily to and from the experiment table, the chicks became so habituated to the transfer that the fear response did not enter in to mar the value of the results. From the first the chicks ate from my hand, and soon many of them energetically followed the hand from point to point, gathering up the bits of food as they were dropped. A little later many even gave the "food twitter" while in my hand on the way from the brooder to the table. But it may not be said with accuracy that the chicks became "habituated" to the operation, if at any age without previous trials they submitted, without signs of being disturbed, to the conditions of the experiment. As a fact, they did not thus submit. Animals that had not been used in the pecking tests nor been handled previously in any other experiment were brought to the table for a control test. They usually struggled when picked up and seemed so disturbed by the situation, when set on the table, that they would not eat at all. This was especially true if they had been allowed to live in the brooder unmolested for two or three weeks.

For the first tests, which were conducted on the second day, because of the physical weakness of the chicks and their indisposition to eat on the first day, slightly moistened bread was used, of such a consistency that it could be rolled between the fingers into food particles of suitable size. After the second day Cyphers Chick Food, slightly moist, replaced the bread pellets in the tests. This, which was the regular food of the chicks, is a mixture of whole wheat, Kaffir corn, cracked corn, millet, etc.

The food dish was kept out of the chicks' field of view and the bits of food to be pecked at were dropped by hand upon the cardboard in such a way that a particle would not be in motion when a chick pecked at it. From one to three grains only were dropped at a time. This proved advisable because the number of reactions elicited by more than three grains, considering also the possible variety and rapidity of the reactions, made it difficult at times to secure an accurate record.

In a letter published by Mills,[1] Bumpus[2] has suggested that the different aspects of the pecking reaction be distinguished. For these separate parts he proposed the terms seizure, mouthing or mulling, and deglutition. The terms used by Morgan[3] seem much more appropriate—striking, seizing, and swallowing. It is interesting to note that Spalding[4] distinguished these phases of the reaction and employed the same terms. These we have adopted, using in addition the term missing for failure to hit the object. As they appear in our records, these terms have the following definite meanings: (1) *Missing* denotes all cases of the pecking reaction in which the bill fails to hit the particular object supplied by the experimenter; (2) *striking*, those cases in which the bill hits the object without seizing it; (3) *seizing*, cases in which the object is grasped momentarily in the bill and then dropped; and (4) *swallowing* denotes what may be termed the perfect or complete reaction, the object being struck, seized, and swallowed in an errorless series or chain of movements. To facilitate the taking of records, the numerals 1, 2, 3, and 4 were used to represent *missed*, *struck*, *seized*, and *swallowed*, respectively. Note was taken, of course, of the reactions independently of the number of food particles pecked at, for a single grain might call forth a half dozen reactions in succession. For example, suppose one millet seed brought the result 1–2–3–4. In this case the chick first missed the grain, on the second reaction it struck it but did not get hold of it between its mandibles, on the third attempt it caught it in its bill but dropped it, and on the fourth, it struck, seized, and swallowed the grain without error.

[1] Mills, W.: The nature and development of animal intelligence. New York, 1898, p. 296.
[2] Bumpus, H. C.: Instinct and education in birds. *Science*, N. S., 1896, vol. 4, p. 213.
[3] Morgan, C. L.: Habit and instinct. London and New York, 1896, p. 37
[4] Spalding, D. A.: *Loc. cit.*

For this, as well as for all other work, the chicks were marked by colored yarns tied on their legs. Each animal thus identified was assigned a number by which it was afterwards known and referred to.

b. Pecking artificially deferred.—The experiments of Spalding,[1] in which chicks on leaving the shell were blindfolded with little hoods or kept in a flannel bag, have attracted much attention from students of instinct. Spalding sought to ascertain the facts in regard to instinct. In his time the skeptical were holding "that all the supposed examples of instinct may be—for anything that has yet been observed to the contrary—nothing more than cases of rapid learning, imitation, or instruction." The hooding device was intended to permit the chicks to acquire "enough control over their muscles to enable them to give evidence as to their instinctive power." In other words, the aim was to test the activity of pecking when the factor of acquisition was eliminated. The conclusions of Spalding as to the accuracy of pecking, previously referred to, are based on the results of these tests.

In the similar tests that are reported below, the purpose was to measure the accuracy of the pecking response under circumstances like the above. Not much success was achieved by hooding the chicks. Other means of excluding the light were relied upon. After being tested the chicks were placed in the brooder with the rest of the flock.

On Dec. 10, 1907, chick no. 8, immediately upon hatching at 1:30 a. m., was blindfolded and transferred to the brooder where it was kept until 10 a. m., Dec. 12, under the curtained hover in a black-lined box at a temperature of 103° F. This box, open at the top, was enclosed in a green flannel bag. At 10 a. m., Dec. 12, the chick was brought carefully to the experiment table for a pecking test. Almost immediately it twice pecked the board upon which it stood. The particular stimulus to the reaction was not apparent. Then followed, in a series, seven reactions, in all of which the chick missed the pellet of bread (rolled as previously described) which elicited them. Seven more pecks at the same crumb followed without hitting it. Then the board on which it stood drew forth two more reactions, when the pecking was interrupted by an awk-

[1] Spalding, D. A.: *Loc. cit.*, p. 282.

ward attempt to bite its wing. After a spell of violent chirping, it missed twice, and hit as often, a spot on the board upon which it stood, following which it lifted a foot to scratch its head and lost its balance. Then four pecks at a crumb missed, but a fifth was successful and the crumb was swallowed—the first perfect reaction. Thereupon came a series of 79 pecking reactions in no one of which a particle of food was hit, seized, and swallowed in a chain reflex. In many of these reactions the head wobbled from side to side as the bill moved slowly toward the object pecked at. On the twentieth reaction in this series of 79, the object was seized in the bill and apparently rejected by the chick. Prior to this, excluding the one perfect reaction, bits of food had been seized on two occasions but dropped through seeming lack of skill. Just preceding an interval occupied in preening the feathers of its wings, an interval which marked the end of the 79-reactions series, there occurred a group of five reactions, in three of which the object was struck and in the other two seized. It is but fair to state that many of the pecking reactions were not in the direction of food particles, there being something about the bare surface upon which the chick stood that drew the reactions forth. In the summary of the chick's record these reactions have been classified with the 1's. At this point it was incidentally noticed that the chick followed with both head and eyes a movement of the experimenter's hand. After the above mentioned preening diversion, twelve more pecking reactions ensued: seven 1's, one 2, and four 3's. Then the animal hesitated long enough to scratch its bill with its foot. The next nine reactions, of which three were 1's, five 2's, and one a 3, came out in straggling order, interrupted by gaping, pecking of toes, scratching the bill, and preening the feathers of the breast.

When tested by a moving object, a moist bit of bread swinging by a black silk thread, the chick's first impulse was manifestly toward it, but later it acted as if afraid. During these tests the chick appeared in no danger, except by accident, of stepping off the edge of the piece of cardboard upon which it was placed, when the cardboard was so arranged that the outer edge of it coincided with an edge of the experiment table. The animal could easily be pushed away from the edge but, when near the edge, *resisted strongly* if pushed toward it. This same

behavior was noticed in other chicks. For example, no. 14, the day after it was hatched, when on the experiment table for the first time, was pushed toward the edge. It resisted by bracing its legs in front of itself and hurried back from the edge as soon as it was released.

The summarized record of no. 8 is as follows:—

1–1–1–1–1–1–1–1–1–1–1–1–1–1–1–1–1–1–1–1–2–2–1–1–1–1–4–1–1
2–1–1–1–1–1–1–1–1–2–2–3–1–2–1–1–3–3–1–1–1–1–1–1–1–1–1–1–
1–1–1–1–1–1–1–1–1–1–1–1–1–1–1–1–1–2–2–1–1–1–1–1–1–1–1–1–
1–1–1–1–1–1–1–1–1–1–2–1–1–1–2–2–2–3–2–2–3–1–1–3–1–1–1–1–1
–2–3–3–3–1–2–2–2–1–2–2–1–3.

Attention has been called to the fact that the chick pecked the cardboard at a place where no particular spot or grain was discerned by the experimenter. Not only that, but, like a child repeating da–da, it sometimes continued with a rapid succession of reactions at or near the same place. Twenty-five of the long series of 1's, immediately following the third 3 in the record above, represent such pecks at the bare card. They came out in three groups, first 2, then 18, and after an interval the other 5. To see a chick hammering in this way 18 times in rapid succession, and hard, too, at apparently "nothing," reminds one of the mere exercise of a function, and suggests what Baldwin has termed the circular reaction.

A comparison, in regard to accuracy, of no. 8's record, with the numerous records which will be presented later, will show that this chick was below the average efficiency in its first test. It is not unusual to find normally kept chicks that do not make a single swallowing reaction in the first twenty trials on their second day, and chicks sometimes fail at this age to seize a single grain in the same number of trials, but I have found only one in the regular tests that failed to strike at least one grain in the first twenty reactions. There were instances in these first tests in which the side of the chick's bill touched the side of the object as the bill slowly passed and hit the cardboard. These were recorded as 1's, here and elsewhere.

Naturally, after being kept 56.5 hours prior to the tests under the conditions previously described, the chick's down was ruffled and it lacked the general sleek appearance of its unconfined mates. Whether it was deficient in anything else but the results

of practice, I cannot say. The confinement did not do it any permanent injury, it seems, for it continued in the pecking tests, lived to make a perfect record after an average number of trials in later black-blue color selection tests, and survived after that till it was killed and dissected for sex determination on March 14, 1908. Its record in the later pecking tests is submitted in table 1. This table shows the accuracy of its pecking during the nine days after Dec. 12, on the basis of twenty reactions a day.

TABLE 1

Chick no. 8. Dec. 13–21, 1907. Pecking

Date	13	14	16	17	18	19	20	21
Missed	2	1			2			
Struck	3	4	7		2	2	2	1
Seized	1	1		3	1			1
Swallowed	14	14	13	17	15	18	18	18

The record of this chick in its first test is not presented as typical, but is submitted to show how a chick may act under conditions like those set forth above.

Two other chicks, no. 7 and no. 14, of the same brood as no. 8, were enclosed at the same time in the dark-box with no. 8. These chicks were helped out of their shells just as they were hatching and transferred at once to the dark-box. They were not hooded, but even if they had been securely blindfolded, little would have been gained, for the light that reached the interior of the black-lined box was negligible in amount.

No. 7 was brought to the light for its tests at 11:05 a. m., Dec. 11, after 33 hours, 35 min. in the dark-box. My notes on its behavior are as follows: "Followed moving object (hand) with head. Pecked wood over edge of black cardboard. Wiped bill on board. Pecked three times at a pellet of bread moistened with milk. Missed it each time. Chick pushed bill slowly toward bread crumb on the black surface—a slow peck. Missed crumb, weak peck. Bit toes. Touched small white speck with bill. (The experimenter did not point at objects.) Rested under hand with its eyes closed. Missed a crumb twice. Missed another. Moved bill toward it slowly. Chick able to stand. Pecked at feet and lost balance. Bit wing twice. Pecked black cardboard. . . . Chick seemed cold and was returned to the brooder." The record of no. 7's pecking at food particles and

spots on the card runs thus in its first test, as detailed above: 1–1–1–1–2–1–1–1. Table 2 shows the accuracy of its pecking from Dec. 13 to Dec. 26.

TABLE 2

CHICK NO. 7. DEC. 13–26, 1907. PECKING

Date	13	14	16	17	18	19	20	21	24	26
Missed					1					
Struck		7	5	2	3	1	7	1		2
Seized	3		3	1	2			1	1	1
Swallowed	17	13	12	17	14	19	13	18	19	17

Chick no. 14, as just stated, had been placed in the same enclosure with no. 7 at the same hour on the same date, and was kept there till 4:25 p. m., Dec. 11. After 38 hours and 55 min. thus enclosed, it was tested in the same way as no. 7. On the first pecking attempt it pushed its bill, mandibles closed, into a bread crumb. Then followed six pecks that missed. Attempted to scratch its head and lost its balance. Seemed weaker than the other chicks. Pecked toes and bare black cardboard; scratched bill. A second time it pushed its bill slowly into a bread crumb. Then followed a reaction in which it missed, etc. The summarized record for this test appears thus: 2–1–1–1–1–1–1–2–1–2–2–1–2–2–2–2–1–3–2–2–2–2–3–2–2–2. The accuracy of its later pecking, Dec. 13 to Dec. 26, is shown in the following table:

TABLE 3

CHICK NO. 14. DEC. 13–26, 1907. PECKING

Date	13	14	16	17	18	19	20	21	24	26
Missed					1					1
Struck	9	6	12	6	5			3	4	1
Seized	2	1		1			1	1	2	
Swallowed	9	13	8	13	14	20	19	16	14	18

A comparison of the records of these three chicks for Dec. 13 with the corresponding records of five others of the same age which were not subjected to confinement in darkness shows that there was no noteworthy difference in the development at this time. The averages of the three chicks on the above date were .7, 4, 2, and 13.3 for reactions 1, 2, 3, and 4, respectively, while the other five chicks averaged .8, 1.4, 4, and 13.8 for the same reactions, in a total of twenty reactions. On the

other hand, the development of no. 8 on Dec. 12 was far behind that of any chick in the above group of five on the same date.

It looks very much as if (1) the development of the instinct was retarded by disuse, and (2) the retardation was quickly overcome with use. Several attempts to repeat these dark-box tests with other chicks failed. On these later occasions the plan was conceived of placing the dark-box inside the incubator just before the expiration of the period of incubation, depositing therein a few of the eggs, and allowing the prospective chicks to hatch and remain in the dark enclosure till needed. For some unforeseen reason the chicks did not appear.

We turn now to a more thorough investigation of the "congenital definiteness" of the pecking reaction, with a view to a quantitative determination of its course of development.

c. The natural development of the pecking reaction, with a collateral study of the effects of social influence. 1. Sources of error.—In addition to what has already been said of the method of taking records, the important sources of error might be mentioned. Investigators have noted the fact that the chick, if it misses the grain at all, usually comes within a hair's breadth of it. Hence, when the animal is working rapidly, it is sometimes difficult to determine whether the bill hits the grain or not. In the work herewith reported, a chick was credited with reaction 2 if there was any doubt between 1 and 2. In other words a 1 was recorded only when the miss was positively observed. The fact that more missing reactions seem to be recorded among the later results may be due rather to modification of the experimenter than to variation in the groups of chicks. I feel quite confident that I became a more efficient observer of the missing reactions as the work progressed.

Again, there are times when reactions 2 and 3 may be easily confounded. A grain seized simultaneously with the strike may almost immediately fly from the bill—like a cherry pit squeezed between the fingers. This reaction, properly 3, may be confused with the reaction where the grain is driven from its position by the mere impact of the bill. Still, as one becomes familiar with such cases, the difficulty of distinguishing them almost wholly vanishes.

Occasionally fewer than the regular number of reactions in the daily series are reported for a given date. In such cases

the cause was usually the refusal of the chick to eat. This difficulty was soon obviated by carefully controlling the amount of food allowed the chicks, and compelling them to scratch for it in the litter. The objection also was thereby forestalled that many chicks under an improperly regulated food supply might react the required number of times but below their highest efficiency.

Not only did these difficulties threaten to affect the reliability of the results, but an additional reaction came in to complicate the record. When the food supply was improperly controlled, the chicks would quite often *reject* grains, that is, lift them from the ground with the bill and then either drop them or throw them some distance without attempt to recover. Forcible rejection is distinctly a reaction in addition to reaction 3. It involves control of the grain by the mandibles—sufficient, one should suppose, for manipulation without error prior to swallowing. If this be true, it is manifestly unfair to charge the chick with a degree of imperfection by recording reaction 2. Yet, one is not at liberty to credit it with reaction 4. A clear instance of rejection should simply not figure in the records. But this disturbing feature was practically eliminated when the chicks were confined in the brooder at night and were given the proper amount of food only after the morning tests. I say practically eliminated, because, even after taking the precaution mentioned, some chicks in time came to eat one kind of grain and not another. For example, I have found an occasional chick that discriminated in this fashion between millet seeds with the shell on and those without the shell, to say nothing of preferences for wheat as against corn, millet as against wheat, and the like. In such cases the chick was fed what it readily ate.

Another variation, which we may term switching of the bill, occasionally appeared. It is similar in character to the forcible-rejecting movement, and was observed in some very young chicks. It is executed while the food particle is held firmly between the mandibles. It is mentioned here rather as an additional type of reaction than as a source of error, for it did not noticeably affect the records.

I have so many times observed chicks, especially very young ones, peck where I could see nothing but bare wood or cardboard, that I am compelled to believe that the stimulus to the

pecking reaction need not be some object of a size convenient for eating. In line with this consideration is the further fact that the bill of the chick is not only a feeding but a *testing* organ. If anywhere, the " oral sense " has its organ here. The chick often digs in the litter with its bill when the reaction is distinctly not the pecking reaction. While trying to escape from the reaction box of a discrimination apparatus in a dark-room, chicks often approached a sheet of clean, smooth, lighted opal glass and pecked it. In the same box they pecked the black walls and especially the black cardboard closing the exit, when they happened to enter the wrong side and found their egress barred. But, except where specially noted, no missing reactions were recorded when no object to be pecked was supplied.

Besides the occasional variation in the missing reaction in which the animal pecked the bare cardboard when no other stimulus seemed to be present, there were two other reactions that were easily distinguishable from each other and from the regular missing reaction, but which were, nevertheless, put down in the records as reaction 1. These variations in the missing reaction were altogether so few in number, compared with the many hundreds of reactions recorded, that the averages are not materially affected by the classification of them with reaction 1. I refer here (1) to cases in which the chick pecked in the direction of the grain but did not reach it, and (2) to cases in which the bill struck the cardboard almost exactly midway between two grains. Of the first of these I have records of thirteen instances; of the second, thirty-six instances. Case one needs no special comment. The bill each time was going in the right direction, but the innervation seemed to be insufficient. Case two is, if anything, more interesting. When the typical instance of it occurred, the grains were usually about 1 cm. apart, of about equal size, and the line connecting the two ran about normal to the direction in which the chick was oriented, the chick standing directly before them. One could say with little certainty that this was an instance of pure missing. We have already agreed with other observers that the chick from the first rarely missed by more than a hair's breadth an object pecked at. But here was a case where a chick from one to four weeks old missed a grain by 4 to 6 mm. Not only that, but I have five records where in a situation like that described above the chicks pecked twice

in succession between the two grains, and one record in which there were four such successive pecks. There are two other observations that help to explain this reaction. On Dec. 23, no. 70 and on Dec. 29, no. 87, were working rapidly in their regular pecking tests. The pecking activity was interrupted momentarily while the bill poised in air above and about midway between two grains approximately 1 cm. apart. On Dec. 24, no. 76 was on the experiment table going through its pecking test. In the presence of two grains lying as those in the preceding cases, its pecking reaction started in the direction of one of the grains and was completed by a perfect reaction upon the other. I concluded, therefore, that case two was not an ordinary instance of missing, but that the reaction in response to one of the stimuli exerted an inhibitory effect on a reaction in response to the other. The poising of the bill in air, the zigzagging first toward one grain and then toward the other during a continuous forward movement of the bill, and the striking of the cardboard fairly between the grains, have at bottom, I believe, the above common principle of explanation.

In view of the recent important work of Hess and that of Katz and Révész, I feel impelled to report some activities of the chicks in the dark-room. The assumption that hungry chicks do eat when they can see the food is ordinarily true, but the second assumption, that chicks do not eat when they cannot see it, surely is not borne out by observations of the behavior of chicks in the dark-room. During some studies of individually acquired reactions, the chicks were placed in a dark-basket in the dark-room for ten minutes prior to the regular daily tests. The basket which was used for this adaptation work was a willow waste-paper basket lined with black cloth and covered with the same material. Food was placed in the bottom of the basket before the basket and chicks were taken into the dark-room. The animals were thus allowed to eat in the basket a short time before the period of darkness adaptation began. Again and again, after the basket was taken into the dark-room, the door closed, all lights turned off, and the basket covered, the animals continued to eat the chick food in the bottom of the basket. I have also gone out of the dark-room and returned at the end of the ten minute period to find the chicks eating. The darkness was so nearly absolute in the room that I could not

see the dark basket while sitting in a chair nearby. The pecking sounds, as the bills of the chicks struck the bottom of the basket, could be distinctly heard. The usual mouthing noise made by the mandibles could be heard also when one held his ear near the side of the basket. And when I put my hand into the basket, I could feel the heads of the chicks rising and falling in the pecking reaction.

Before formulating a conclusion let me describe the behavior of no. 71. This chick, in the pecking reaction tests, made the following record from its second to its eleventh day, and then ceased to peck at the grains when brought to the experiment table:

TABLE 4

CHICK NO. 71. DEC. 3–12, 1908. PECKING

Date	3	4	5	6	7	8	9	10	11	12
Missed	13	2	3	2	2			2		
Struck	6	25	32	26	22	8	17	6	12	43
Seized		9	5	3	2	3	3	3	1	1
Swallowed	1	14	10	19	24	39	30	39	37	6

On Dec. 13 it staggered about in a peculiar way, but did not eat. It would walk directly off the table if not prevented. When tested with water in a watch glass, it drank when the water was held to its bill, but after the dish was moved three or four centimeters away, it dipped its bill about where the dish had been. Such indications as these convinced me that the animal was blind. For a day or two it was fed by having grain introduced into its bill. Then it was placed in a large dish partly filled with chick food, and on Dec. 16 it began to scratch and eat. For many days thereafter it was regularly placed in the food dish at the feeding hour and procured its meal without assistance. When I entered the room and the rest of the flock ran toward the wires of the cage, no. 71 was often seen approaching, running into obstacles in its path, bobbing up and down in a peculiar variation of the pecking movement, and taking time intermittently to scratch in the litter. In this fashion it lived until it was found dead one morning in the drinking vessel. The behavior of the chicks in the dark basket, as well as that of this supposedly blind chick, furnish quite clear evidence in refutation of the assumption that chicks do not peck at or eat food when they cannot see it.

But happily the reaction in which we are most interested, reaction 4, can be distinguished from all the others with practically no liability of error. Even supposing a few 1's, 2's and 3's were to become interchanged, the use of reaction 4 as a quantitative index of development would not be invalidated. The main interest is to discover the ratio of the number of perfect reactions to the whole number of legitimate pecking reactions in the daily series, and to plot the variation of this ratio. The necessary data for this can be gathered with precision.

2. Data and their significance. The first animals tested were numbers 1 to 6, inclusive, which will be referred to as

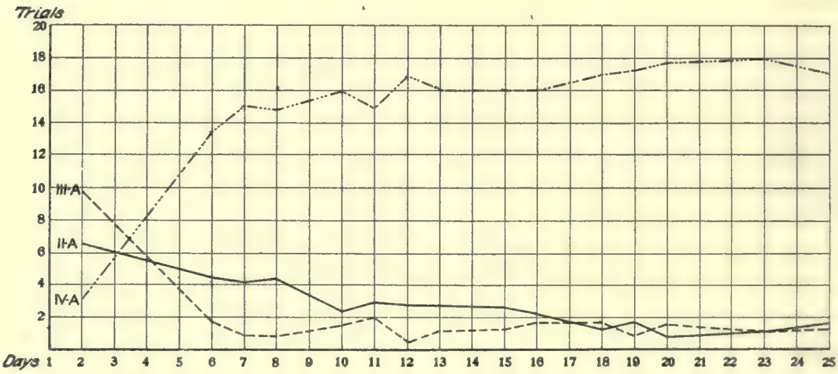


FIGURE 1—The development of the pecking instinct in the chicks of Group A, numbers 1 to 6, inclusive. Distances along axis of abscissae represent days after hatching; distances along axis of ordinates, the number of a given kind of reaction in a series of twenty pecking reactions. Curve II-A shows the rate of decrease in the daily number of reaction 2; Curve III-A, the same for reaction 3; Curve IV-A shows the improvement in accuracy of reaction 4 for the group.

Group A. They were hatched on Dec. 2, 1907. Their pecking records cover the period from Dec. 3 to 30, inclusive. Curve IV-A, fig. 1, shows the development of reaction 4 for the group. For the calculations only those totals were used in which every chick had a complete series of twenty reactions. Hence a very interesting part of this curve, namely, the detail for the third, fourth, and fifth days, is lacking. The blanks are due to the failure of some of the chicks on each of these days to respond to the food. Improper regulation of the food supply was the cause. Nevertheless, the results were such as to encourage a continuance of the experiment. Barring the vital omissions at

the most critical stage of development, the curve on the whole not only indicates that the co-ordination of movements that constitute reaction 4 has a certain period of development, but it roughly marks out that course. The rate of improvement is not uniform from day to day, but is very much more rapid during the first few days than later.

Before discussing reactions 1, 2, and 3 of this group, let us be clear as to the meaning of these three reactions and their relation to 4. In the first place, it should be noticed that the records under 1, 2, and 3 may be regarded as error records. That is, under 1 are listed the errors of striking, under 2 the

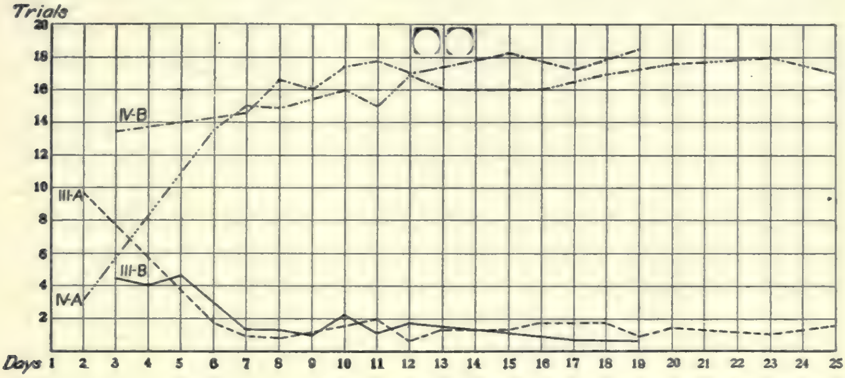


FIGURE 2—A comparison of curves of development of the pecking instinct to show the possible effect of social influence. Distances along axis of abscissae represent days of age; distances along axis of ordinates, the number of a given kind of reaction in a daily series of twenty pecking reactions. Curve IV-A shows the improvement in accuracy of reaction 4, Group A; Curve IV-B, the improvement in accuracy of the same reaction for Group B. Curve III-A shows the course of reaction 3, Group A; Curve III-B, the course of reaction 3, Group B.

errors of seizing, and under 3 the errors of swallowing, if swallowing here may designate the manipulation of the object after it is seized. Although for convenience the term swallowing has been applied to reaction 4, it is clear that reaction 4 is really striking-plus-seizing-plus-swallowing in an errorless train.

There are always three regular chances of error, then, at the beginning of any pecking reaction. I shall aim to show the rate of decrease of each kind of error separately, and, correlatively, the improvement in the co-ordination of the three reactions.

Curve II-A, fig. 1, shows the course of reaction 2, and Curve III-A of reaction 3, for the chicks of Group A. The errors of swallowing tend to vanish more rapidly and more completely than those of seizing. The data on reaction 1 are too meager to justify consideration here.

Curve IV-B, fig. 2, is the curve of development of reaction 4 in Group B, chicks nos. 9 to 13, inclusive. These chicks were hatched just eight days after those of Group A. The two groups were segregated until chicks nos. 9 to 13 were beginning their seventh day, and then were allowed to mingle. The pecking tests were made throughout in the regular way, both groups being treated as nearly alike as possible. Curves IV-A and IV-B, fig. 2, are matched for corresponding days in the lives of the chicks—that is, for corresponding ages, and not for the dates on which the records were made. Although it was not part of the plan, when the experiment was conceived, to study social influence, the higher curve for Group B indicates that possibly association with the older chicks had the effect of facilitating improvement in the younger ones, and suggests a method of measuring this influence. A glance at Curve IV-A, fig. 2, shows that the efficiency of the chicks of Group A on their fifteenth day, when those of Group B joined them, was numerically 16 on a scale of 20, and that the efficiency of Group B at the same time, the seventh day for the latter, was 14.6. But the difference in the curves may be due to unsuspected irregularities in method, or to variability in the chicks. It seemed hazardous, therefore, to rest such an important conclusion on one test, and a repetition of the experiment was planned and executed.

Curves IV-A and IV-B indicate, then, that there is a very rapid improvement in the integration of the components of the complete reaction within the first three days, followed by a slower but fairly steady improvement after that for some time. It is interesting to note just how nearly the development approximates perfection,—18.4 on a scale of 20 representing the degree of approximation at the highest point.

Curves III-A and III-B, fig. 2, trace the course of reaction 3 for Groups A and B respectively.

The study of the pecking reaction was resumed almost a year later with chicks nos. 57 to 65, inclusive, which we shall style Group C. These chicks were hatched Oct. 25, 1908. With a

desire to continue the study of the effect of association of younger with older chicks, eggs were set with the intention of having another brood eight days after Group C, so that the difference between the ages should correspond exactly with the difference in age between the chicks of Groups A and B. But the plan was frustrated to this extent, that the chicks of Group C came out of the eggs one day before the date on which they were scheduled to arrive, and a sudden change of temperature so chilled the second setting that only two healthy chicks arrived to constitute Group D. It was decided to make the most of the opportunity, however, and records were taken of the pecking

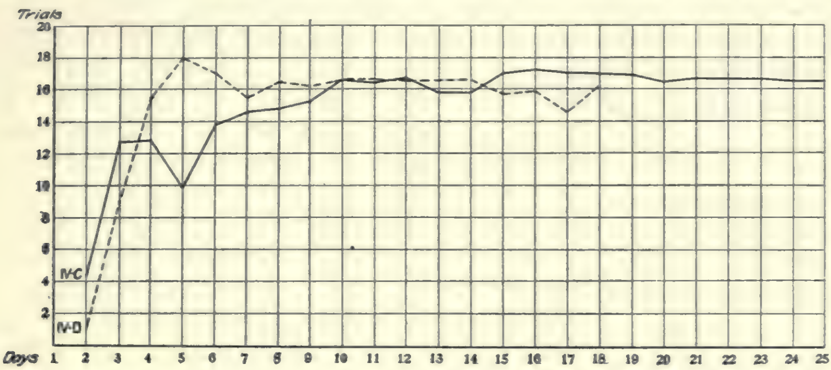


FIGURE 3—A comparison of curves of development of the pecking instinct to show the possible effect of social influence. Distances along axis of abscissae represent days of age; distances along axis of ordinates, the number of "perfect" reactions in a daily series of twenty pecking reactions. Curve IV-C shows the improvement in accuracy of pecking in Group C; Curve IV-D, the improvement in Group D.

of the nine chicks in one group and the two in the other. When the members of Group D were beginning their eighth day, the cages of the two groups were thrown together and the chicks allowed to mingle. For three days, beginning with this day, 100 pecking reactions per day were recorded for each chick in both groups; and, continuing for a week after this, 50 reactions a day were taken. It was thought that by this means the influence, if any, of one group upon the other might be more easily and accurately detected. The results are plotted in fig. 3, and the curves matched for corresponding ages as before. The chicks of Group C had a pecking efficiency of 17.1 at the beginning of their seventeenth day when those of Group D joined

them. On this same date, when the individuals of Group D were eight days old, they had an efficiency of 16.5. Very little change occurred in the accuracy of pecking of Group D after the commingling, nor indeed could a heightening of their curve after the eighth day be well expected, for they had by that time practically attained the maximum.

Nothing very conclusive having accrued here, it was deemed desirable to arrange a test so that younger chicks could be enclosed with older ones immediately after hatching. Thus, it was thought, the influence might be measured, if the effect of social influence be such as to affect the rate of improvement of the pecking reaction. The most active opponents of inferential, ideational, or voluntary imitation seem agreed that the presence of one animal, under the proper conditions, may have the effect of stimulating another of its kind to greater activity. There is no difficulty whatever in establishing this fact. My observations have proved it to my satisfaction many times. It may be that the change in the mode of functioning of the organism due to the presence of others is in the direction of increasing the intensity and rapidity of the reactions, without increasing their rate of improvement in accuracy. Such a variation would have selective value, inasmuch as the animal would get more food in a given time, even though the pecking were no more accurate. But such a speculation does no more for us than open up wider vistas of experimental research in which exact quantitative studies can no doubt be made.

Mention has previously been made of the rapidity with which reaction 4 improves. The curves all show very rapid improvement on the second day. To make a special measurement of the rate of this change, the pecking of six chicks in Group C was tested twice on the second day. The first record was taken from 10–12 a. m. and the second from 2–4 p. m. In the interim the chicks were committed to the brooder and allowed the freedom of the litter of the cage for the first time. From forenoon to afternoon, within the limits just stated, the improvement was 82%, assuming, as we have, that the development of reaction 4 is an index of the improvement of the chicks' pecking efficiency.

The sudden drop in Curve IV-C, fig. 3, on the fifth day is due mostly to the unusually poor records of no. 57 and no. 58

on that date. Twelve of no. 58's striking reactions were at one particle. It followed this up, hitting it time after time until the grain was knocked off the table.

The final test of social influence in its relation to pecking was made on two comparable groups of six chicks each, which will be referred to as Group E and Group F. The chicks of Group E were hatched on Dec. 2, 1908; those of Group F on Dec. 12, 1908. The members of Group F, thus ten days younger, were transferred from the incubator to the cages and brooder occupied by Group E. Records were taken as before. Still greater precaution was observed as to food supply. The food was not

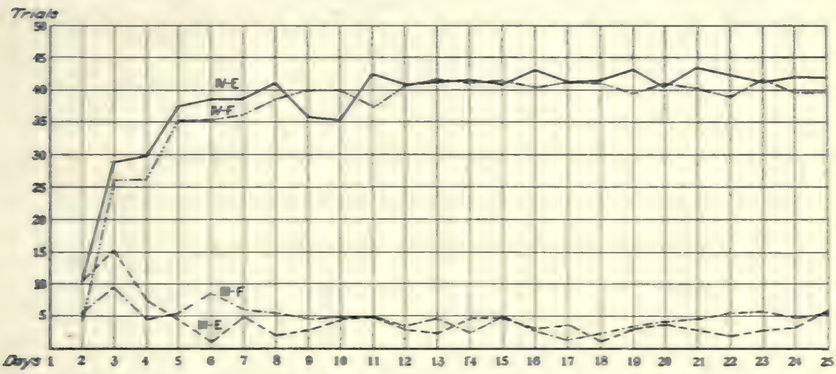


FIGURE 4—A comparison of curves of development of the pecking instinct to show the possible effect of social influence. Distances along axis of abscissae represent days of age; distances along axis of ordinates, the number of a given type of reaction in a daily series of fifty pecking reactions. Curves III-E and III-F show respectively the rate of decrease in the number of reaction 3 for Groups E and F; Curves IV-E and IV-F show the improvement in accuracy of reaction 4 for the same groups.

unnecessarily stinted, however. The chicks chirped loudly when I came into the room in the morning and crowded toward me when I approached the side of the brooder. In the tests they picked up the grains very energetically.

The records for Group E are given in full in table 5, the summary for Group F in table 6. The curves for reactions 3 and 4 of both groups are plotted in fig. 4; for reactions 1 and 2 in fig. 5. The efficiency of the pecking of the older group is represented numerically by 42.2 on a scale of 50 on their eleventh day, when the chicks of Group F entered the cage. The twelve chicks were confined in the brooder each evening together, were

TABLE 5

Pecking of Chicks nos. 69, 70, 72, 73, 76, and 79, Group E. Dec. 3–26, 1908

Chicks	Date	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
No. 69	Missed...	8		1	2		1				1	3	4				1			2	2		3		
	Struck...	7	5	8	6	7	4	2	8	3	5	7	7	4	5	5	6	9	2	5	2	4	5	5	4
	Seized....	1	4	5	1	4	1	3	4	4	2	2	1	3	5	2	4	1	2	3	3		1	3	6
	Swallowed	4	41	36	41	39	44	45	38	43	42	38	38	43	40	43	39	40	46	40	43	46	41	42	40
No. 70	Missed...	2											1	1	1		1	4		1	2	1			
	Struck...	10	4	7	7	5	5	4	4	8	2	6	6	5	3	4	3	8	7	3	3	5	9	4	1
	Seized....	8	32	14	4	3	6	2	3	5	6	7	3	8	6	5	8		3	4	4	4	1	3	7
	Swallowed		14	29	39	42	39	44	43	37	42	37	40	36	40	41	38	38	40	42	41	40	40	43	42
No. 72	Missed...	1										1													
	Struck...	4	3	9	1	4	1		2			2	3	2	2	1	3	2	4	2	1	2	1	1	2
	Seized....	2	10	3	1		7		4	4	3	4	2	5	2	5		2	2	5			2		3
	Swallowed	13	37	38	48	46	42	50	44	46	47	43	45	43	46	44	47	46	44	43	49	48	47	49	45
No. 73	Missed...	7	1	2			2			1				1		3	1	1			1	1			
	Struck...	7	7	31	18	14	10	17	8	15	8	8	7	1	8	3	4	8	3	13	8	6	6	5	2
	Seized....	6	17	6	5		4	1	2	5	8	2	3	6	9	3	4	3	5	3	3	3	6	8	11
	Swallowed		25	11	27	36	34	32	40	29	34	40	40	42	33	41	41	38	42	34	38	40	38	37	37
No. 76	Missed...	4	4			1																			
	Struck...	3	12	12	8	11	3	4	5	4	1	7	7	6	3	2	3	3	5	4	3	5	4	10	1
	Seized....	5	21	14	10	1	7	1	3	7	3	1		4	3	4	4	2	3	5	4	3	4	4	6
	Swallowed	8	13	24	32	37	40	45	42	39	46	42	43	39	44	44	43	45	42	41	43	42	42	36	43
No. 79	Missed...	4				1	5		6									1							
	Struck...	13	3	7	10	16	11	12	31	31	2	3	1	2	6	3	7	4	3	5	1	10	7	4	4
	Seized....	3	7	4	3	3	3	6	4		6	1	6		2	1	2	1	3	3	3	2	2	1	3
	Swallowed		40	39	37	30	31	32	9	19	42	46	43	48	42	46	41	44	44	42	46	38	41	45	43
	Age	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25

Totals 1	(65)	5	3	2	2	8		6	1	1	4	5	2	1	3	3	6		3	5	2	3		
2	(110)	34	74	50	57	34	39	58	61	18	33	31	20	27	18	26	34	24	32	18	32	32	29	14
3	(62)	91	46	24	11	28	13	20	25	28	17	15	26	27	20	22	9	18	23	17	12	16	19	36
4	(62)	170	177	224	230	230	248	216	213	253	246	249	251	245	259	249	251	258	242	260	254	249	252	250
Averages 1	10.8	.8	.5	.3	.3	1.3		1.	.2	.2	.7	.8	.3	.2	.5	.5	1.		.5	.8	.3	.5		
2	18.3	5.7	12.3	8.3	9.5	5.7	6.5	9.7	10.2	3.	5.5	5.2	3.3	4.5	3.	4.3	5.7	4.	5.3	3.	5.3	5.3	4.8	2.3
3	10.3	15.2	7.7	4.	1.8	4.7	2.2	3.3	4.2	4.7	2.8	2.5	4.3	4.5	3.3	3.7	1.5	3.	3.8	2.8	2.	2.7	3.2	6.
4	10.3	28.3	29.5	37.3	38.3	38.3	41.3	36.	35.5	42.2	41.	41.5	41.8	40.8	43.2	41.5	41.8	43.	40.3	43.3	42.3	41.5	42.	41.7

TABLE 6

AVERAGE DAILY NUMBER OF EACH KIND OF PECKING REACTION FOR THE SIX CHICKS OF GROUP F

Age	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
Missed	20.	1.5	.3	.2	.2	.2	.7	.3	0.	1.	.2	.5	.7	.3	.2	0.	1.	.2	.7	0.	.5	.5	.3	.3
Struck	20.3	12.7	18.3	9.2	6.7	7.3	5.8	5.2	5.2	7.	5.5	3.7	5.5	3.5	6.2	7.3	5.3	7.	4.5	5.	5.8	2.3	5.7	5.2
Seized	5.3	9.	4.5	5.2	8.	6.3	5.2	4.5	4.8	4.7	3.5	4.2	2.7	4.7	3.2	1.2	2.2	3.2	4.	4.5	5.2	5.3	4.8	5.2
Swallowed	4.2	26.7	26.8	35.5	35.2	36.3	38.2	40.	40.	37.3	40.8	41.7	41.2	41.5	40.5	41.5	41.3	39.7	40.8	40.5	38.5	41.9	39.2	39.3

given their tests at the same period in the morning, and of course were fed together in the same litter, both groups being compelled to scratch for their food. Yet the similarity of the curves for the first eight days, when modification progresses most rapidly, is conspicuous, the curve for the younger brood running slightly lower. These young chicks, in spite of the examples of more accurate pecking furnished them by the behavior of the chicks of Group E, began less accurately than their elders, remained behind them by about the same margin during the critical period of development, and hardly equalled them while the experiment continued.

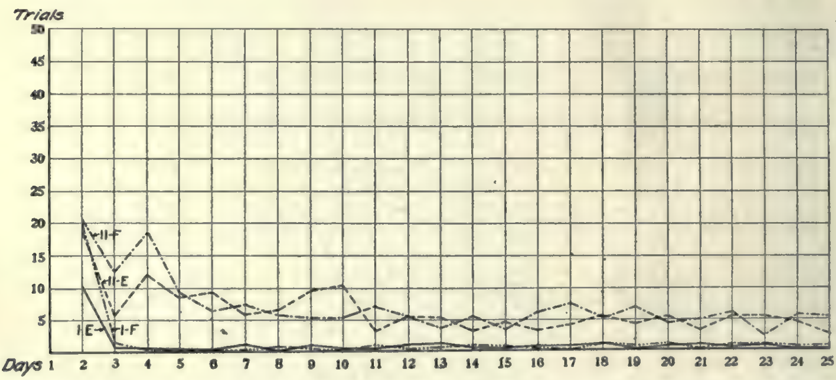


FIGURE 5—A comparison of the curves of development of the pecking instinct to show the possible effect of social influence. Distances along axis of abscissae represent days of age; distances along axis of ordinates, the number of a given type of reaction in a daily series of fifty reactions. Curves I-E and I-F show the rate of decrease in the number of reaction 1 for Groups E and F, respectively; Curves II-E and II-F, the rate of decrease in the number of reaction 2 for the same groups.

A feature worthy of notice is the fact that development of reaction 4 in both groups seemed to halt on the third day, with very rapid improvement preceding and following. A search for the cause of this retardation of development in the chain of actions is interesting. A detailed examination of the relations of Curves I, II, III, and IV for both groups brings out the following points:

1st day, the course of development is unknown;

2nd day, striking and seizing improve rapidly, while swallowing (in the restricted sense) declines considerably in effectiveness;

3rd day, swallowing improves rapidly, striking improves slightly, and seizing suffers a reversal of form.

In other words, from the beginning of the second day the accuracy of striking improves uninterruptedly, the accuracy of swallowing declines temporarily on the second day, and that of seizing temporarily on the third day. It is evident that the rate of improvement of reaction 4 depends upon the rate of decrease of the total number of errors in these reaction types. In neither Group E nor Group F was improvement in all three types of reaction uninterrupted. The retardation of reaction 4 on the third day is due specifically to a decline in the accuracy of seizing on that day.

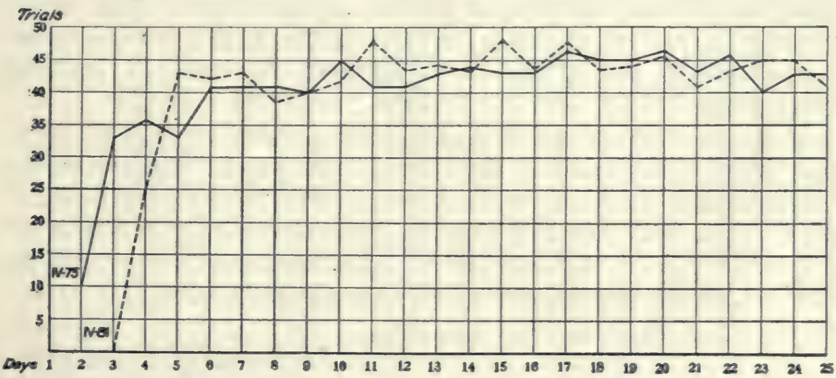


FIGURE 6—Curves showing the rate of improvement in pecking accuracy of two chicks kept in isolation. Distances along axis of abscissae represent days; distances along axis of ordinates, the number of perfect reactions in the daily series of pecking reactions.

In regard to the tests for social influence, the objection can be made that social influence, if it affect the accuracy of reaction at all, may work as well between members of the same group as between members of different groups. The objection has force. As a control test, chicks nos. 75, 80, and 81 were kept in individual apartments and tested. The results are exhibited in fig. 6. The record of no. 80 is omitted because valueless. This chick soon became physically unfit. No. 75, although it did not thrive under the conditions, made a good pecking record. No. 81 not only pecked with unusual accuracy but was lively and vigorous after the first few days. The curves do not furnish convincing evidence in support of the view that social

influence accelerates improvement in the accuracy of the reaction studied.

The curve for no. 81 clearly suggests retardation of development—a very interesting fact, if due to the isolation. At first this animal seemed quite indifferent to food. Besides, its toes were bent under in such a way that it looked crippled. In a few days, however, neither of these failings was observable. They may indicate that the chick was slow in the process of unfoldment in more respects than in pecking. It would hardly be fair, for example, to attribute the slow expansion of the toes to the isolation. No. 81 turned out to be a fine animal. The rapidity with which the components of reaction 4 became integrated, once the process set in, is instructive. The maximum of accuracy attained, as well as the high average maintained, mark the nicety of adjustment in no. 81's pecking mechanism.

If chicks that are isolated peck with a normal degree of accuracy within the natural time limit, and chicks stimulated by the presence of others much more efficient than themselves fail to show a supernormal rate of improvement, one has good grounds for believing that the social influence, whatever else its effect, does not appreciably modify the natural course of development of the pecking concatenation.

In fig. 7 are presented the curves of development of the pecking reaction, based on the averages obtained from the records of the twenty-one chicks in Groups C, E, and F during their first twenty-four days. The data for these curves are given in table 7. Curves I, II, and III, standing for reaction 1, 2, and 3, respectively, represent, as explained before, the distribution of errors. Curve IV represents reaction 4. The sum of the heights of the error curves above the base line on any given day will equal the distance of Curve IV below its limit for the same day.

"In nearly all cases, as one might expect," says Morgan,[1] "the simple process of striking is more accurate than the more complicated process of striking and seizing; and this, again, than the yet more elaborate process of striking, seizing, and swallowing." From the data in table 7 it is an easy matter not only to substantiate this assertion, but to show the quanti-

[1] Morgan, C. L.: Habit and instinct. London and New York, 1896, p. 37.

TABLE 7

PECKING AVERAGES FOR 21 CHICKS FROM THE DAILY TOTALS FOR REACTIONS 1, 2, 3, AND 4 OF GROUPS C, E, AND F

Age	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
											REACTION 1													
Group C.....	160	37	22		2				7	7	12	2	7	12		1	1	2	2	3		2	5	1
" E.....	65	5	3	2	2	8		6	1	1	4	5	2	1	3	3	6		3	5	2	3		
" F.....	120	9	2	1	1	1	4	2		6	1	3	4	2	1		6	1	4		3	3	2	2
Totals...	345	51	27	3	5	9	4	8	8	14	17	10	13	15	4	4	13	3	9	8	5	8	7	3
Averages.	16.43	2.43	1.28	.14	.24	.43	.19	.38	.38	.67	.81	.48	.62	.71	.19	.19	.62	.14	.43	.38	.42	.38	.33	.14
											REACTION 2													
Group C.....	115	25	35	162	95	110	115	92	65	60	40	67	82	27	47	46	58	61	68	51		67	69	70
" E.....	110	34	74	50	57	34	39	58	61	18	33	31	20	27	18	26	34	24	32	18	32	32	29	14
" F.....	122	76	110	55	40	44	35	31	31	42	33	22	33	21	37	44	32	42	27	30	35	14	34	31
Totals...	347	135	219	267	192	188	189	181	157	120	106	120	135	75	102	116	124	127	127	99	67	113	132	115
Averages.	16.52	6.43	10.43	12.71	9.14	8.95	9.	8.62	7.48	5.71	5.05	5.71	6.43	3.57	4.86	5.52	5.90	6.05	6.05	4.71	5.58	5.38	6.29	5.48
											REACTION 3													
Group C.....	80	102	105	65	40	17	10	15	7	17	22	25	5	25	10	17	8	9	11	14		5	9	11
" E.....	62	91	46	24	11	28	13	20	25	28	17	15	26	27	20	22	9	18	23	17	12	16	19	36
" F.....	32	54	27	31	48	37	31	27	29	28	21	25	16	28	19	7	13	19	24	27	31	32	29	31
Totals...	174	247	178	120	99	82	54	62	61	73	60	65	47	80	49	46	30	46	58	58	43	53	57	78
Averages.	8.29	11.76	8.48	5.71	4.71	3.90	2.57	2.95	2.90	3.48	2.86	3.10	2.24	3.81	2.33	2.19	1.48	2.19	2.76	2.76	3.58	2.52	2.71	3.71
											REACTION 4													
Group C.....	95	285	287	222	312	322	325	342	370	365	375	355	355	385	392	385	381	377	369	382		376	367	368
" E.....	62	170	177	224	230	230	248	216	213	253	246	249	251	245	259	249	251	258	242	260	254	249	252	250
" F.....	25	160	161	213	211	218	229	240	240	224	245	250	247	249	243	249	248	238	245	243	231	251	235	236
Totals...	182	615	625	659	753	770	802	798	823	842	866	854	853	879	894	883	880	873	856	885	485	876	854	854
Averages.	8.67	29.29	29.76	31.38	35.86	36.67	38.19	38.00	39.19	40.10	41.24	40.67	40.62	41.86	42.57	42.05	41.90	41.57	40.76	42.14	40.42	41.71	40.67	40.67

tative relations involved. E.g., on the second day, out of fifty trials the object was struck, on an average, 33.48 times on the first attempt. Of this number only 16.96 included seizing; and of the latter only 8.67 were reactions in which the object was struck, seized, and swallowed. As the chicks advanced in age, each of these sets of figures approximated the same limit, but naturally they preserved the original order as to size, for with each additional reaction in the series there was an added source of error.

Reverting to the earlier consideration that Curves I, II, and III may be regarded as error curves, an examination of these

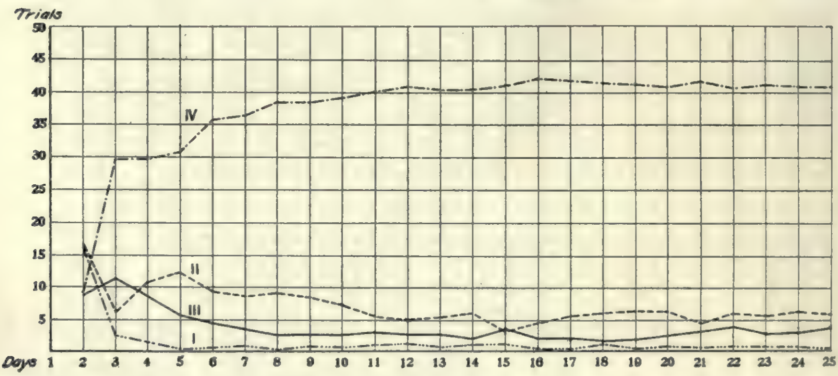


FIGURE 7—Curves of development of the pecking instinct, based on the averages obtained from the records of twenty-one chicks. Data in table 7. Distances along axis of abscissae represent days of age; distances along axis of ordinates, the number of occurrences of a given type of reaction in a daily series of fifty pecking reactions. Curves I, II, and III trace the course of reactions 1, 2, and 3, respectively. Curve IV shows the average improvement in accuracy of reaction 4.

curves shows that the improvement of pecking accuracy is retarded more by failures in seizing than by failures in swallowing; and more by failures in swallowing than by failures in striking. After the third day, the difficulty of seizure remains greater than that of the other two reactions combined. The striking reaction improves in accuracy rapidly and without relapse, closely approximating perfection by the fifth day—a degree of accuracy that might easily inspire belief in the perfection of instinct, for, indeed, this is the reaction which has no doubt been central in the earlier discussions when accuracy has come into question.

Of course these averages hide the unusual performances of individuals. Chick no. 72, for instance, missed only once out of fifty reactions on its second day, on the fifth day it struck, seized, and swallowed forty-eight grains in fifty trials, and on the eighth day devoured every one of the fifty without a slip. The complete record of this animal is given in table 5. A comparison of its record with that of no. 70 brings out nicely the point of individual differences in accuracy. No. 72 developed more rapidly than no. 70, and also attained a much higher average degree of efficiency. The records of both chicks are consistent throughout. They bespeak a more finely adjusted mechanism in the one case than in the other. If we were seeking points of difference instead of identity in this investigation, we should dwell more at length on variations in other respects. Chicks, as one might expect, differ definitely and consistently from day to day in the accuracy of their reactions, in the rapidity of their movements, in their food preferences, and the like, much as men do.

To return to fig. 7. The curve representing the course of development of the complete co-ordination, Curve IV, reaches its highest elevation during the twenty-four days at 42.5 on a scale of 50. Each of these curves is intended to represent an aspect of the pecking of the chicks under the conditions described. Whether or not the reactions in the litter were more accurate than those on the harder cardboard, I cannot say, not having made any measurements. To be sure, the long periods of practice were spent in pecking under conditions quite different from those that obtained in the tests.

It has been the aim of this investigation to discover the meaning that should attach to such expressions as the accuracy, the perfection, or the congenital definiteness of the instinctive action that has been generally considered one of the most perfect, namely, the pecking reaction of chicks. Morgan and Thorndike are correct in asserting that this reaction is imperfect at birth—not " very nearly " perfect as Morgan thinks, but very imperfect. But Thorndike [1] doubts the *fact* of improvement. "As a matter of fact," he writes, " the pecking reaction may be as perfect at birth as it is after 10 or 12 days' experience." I shall have to

[1] Thorndike, E. L.: The instinctive reaction of young chicks. *Psych. Rev.*, 1899, vol. VI, p. 285.

side with Morgan here and insist that the pecking reaction improves in accuracy after birth. Whether, now, this is a case of perfecting through habit, involves a still further problem, that of the relation of habit to the instinct. One sometimes speaks of the *modifiability* of an instinctive action like that of pecking, but wherever this term has been employed in this paper in connection with instinct no more has been implied than the objective fact of improvement in accuracy, an increasingly successful adjustment of parts in a more comprehensive function. The problem still remains, Is this development dependent upon practice, or is it the natural functional correlate of structural maturation independent of practice? Swallows are reported to be able to fly without previous practice. If the pecking of chicks could be successfully inhibited for a week's time without doing violence to the normal physical condition of the animals, would the accuracy of the reactions at the end of that time average 36.67 on a scale of 50, the average for our lot of twenty-one? There is evidence in support of the belief that such chicks would very quickly be pecking with average efficiency, without anything like the amount of practice chicks would have had by this time when growing naturally. In other words, improvement does not depend entirely upon practice. How much of the improvement does depend upon practice? All of it, one is led to believe from Morgan's pages. " Steadying of the inherited organic apparatus " through preparatory efforts means improvement through habit. Besides, overestimating, as he did, the degree of perfection of the instinct at birth, he has left less room for maturation and the effects of practice than there really is. So far as the facts are concerned, the most one can say is that the development of the pecking instinct proceeds somewhat without practice and is hastened by it. Maturation and use run along in time together. No means has yet been devised of measuring the amount either factor apart from the other contributes to the development of the pecking reaction.

The importance attached to individual acquisition as a factor in development seems on the whole to becomê increasingly restricted. The theory of the non-transmission of acquired characters enormously narrows its scope in phylogenetic development. Perhaps we shall discover its lesser importance in ontogenetic development. The drinking reaction, for example, is

more self-dependent than we have hitherto supposed. The pecking reaction suffers the supplementation of habit, one may well believe, when it does not demand it. It remains to be shown to what extent these instincts are typical of instincts in general. But even if it should be established that acquisition contributes relatively little, environment would still remain a powerful factor in development. The animal begins life with an hereditary endowment in interaction with the environment. One is necessary to the other. In the economy of an organism there is no reaction without stimulation. And not only is the environment a system of energies without which natural tendencies of the organism cannot be realized, but it is a selective system. What tendencies shall be realized will be determined by what stimuli are provided. Heredity and environment are not opposites, but complements.

PART II. ACQUIRED REACTIONS

I. Introduction and Statement of the Problem

From the study of the modifiability of reactions that are made prior to experience and are supposed to be based on inherited neuro-muscular co-ordinations, our interest now shifts to a study of the development of reactions that depend upon no such hereditary dispositions in the nervous system, but are individually acquired. It was one of the aims of this investigation to discover not only whether reactions to certain optical stimuli or stimulus-complexes are modifiable, but also to determine the rate of modification; in other words, to describe the progress of habit-formation in quantitative terms.

Modifiability was tested by the so-called discrimination method. Never more than two possibilities of selection were offered to the animal at one time. The conditions were so arranged that the chick's natural tendency to react to confinement and solitude by efforts to escape furnished the necessary random activity. When, after a certain number of trials, the animal reacted selectively to one of two constant form or size or color stimuli, for neither of which it displayed a preference before training began, the process of habit-formation was adjudged completed. By selective reaction is here meant the ability to react without error to a constant stimulus for an arbitrarily fixed number of times.

The adoption of the discrimination method followed directly from the results of Yerkes' work with the dancing mouse. I have appropriated his method, my contribution being merely an adaptation of the method to the study of chicks. So much for the problem in a general way.

II. Apparatus

The apparatus used was a fan-shaped reaction box built of wood and painted brown. Fig. 8 shows the ground plan, fig. 9 the perspective. The method of construction was as follows. On a base 88 by 95.5 cm. a convenient point near one edge was selected from which as a center, with radii of 38.64 cm. and 69 cm., respectively, arcs of circles were described. This center later became the point from which the chick started at each trial, and around which the entrance box was built. The nearer arc marked the location of the cards first approached by the chick; the more distant one outlined a boundary of the apparatus. The box was made with four compartments, but only the two inner ones were used in these experiments. Removable partitions (J, fig. 8) made possible this use of only a limited portion of the box. The height of the apparatus was 20 cm., inside measurement. The covering consisted of two lifting doors, one over the entrance box A and the other over the rest of the apparatus. These were made of light wooden frames and wire netting, and were so arranged that they lifted on their hinges directly away from the experimenter as he stood at the entrance box. The mesh of the netting was 3 cm.—as large as conditions would permit, so as to interfere with the light as little as possible. The exits H were closed with removable galvanized sheet iron sliding doors that fitted from above in vertical grooves. The end of the box about G was closed with wire netting of .86 cm. mesh. Grooves at this end also provided for wooden screens inside the wire of sufficient height to conceal the flock of chicks in a cage just beyond and below the experiment box from the chick reacting. Two sets of cards were used, one at D and another at F. The card-carriers at F projected 2.55 cm. from the walls, making it impossible for the exit H to be seen from the chamber C.

Following the ground plan in fig. 8 and the path of a chick through the apparatus from the entrance box A, the remaining

dimensions and details may be clearly supplied. From the entrance box A, 16 by 20 cm., the chick was released by a wire screen door of .86 cm. mesh that was raised and lowered in its grooves at B by the experimenter. This retaining screen was shaped so that its lower edge coincided with an arc of a circle constructed from the center above referred to. The width of the opening at B was 10 cm. The first cardholders were at D, the passage ways through which were 9.33 cm. wide. Through

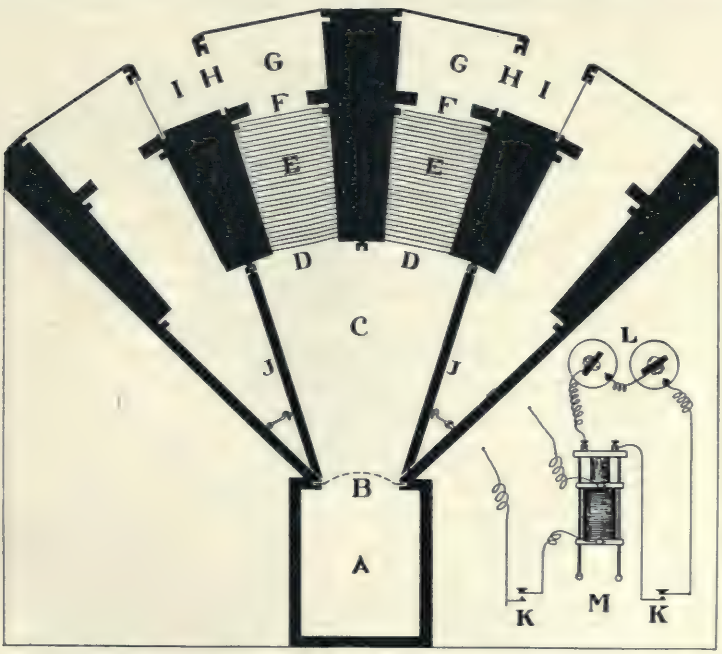


FIGURE 8—Ground plan of apparatus for testing habit-formation. A, entrance box; B, wire-screen door; D and F, location of card-holders; E,E, electric chambers; G, H, I, way out of box; K, electric key; L, electric battery; M, inductorium.

D the chick passed into the electric chamber E, the floor of which was crossed with copper wires 1 cm. apart, so arranged that they formed an interrupted circuit which could be closed by the chick when the current was on by its stepping simultaneously on two adjoining wires. Out of E the chick passed through the opening F, 8.5 cm. wide, the position of the second card. From G there was an outlook through the wire netting

upon the flock of chicks in the cage below and also an outlet through H which was 8.5 cm. wide. The door of galvanized sheet iron could be inserted here to close the exit when desired. Once in I, the chick joined the rest of the flock by passing down over the door of the cage that was turned up toward this end of the apparatus at an angle of about 45 degrees. The dimensions and details were the same by either of the routes a chick might take from A to the cage. K indicates a key by which the circuit was made and broken. The current was supplied by

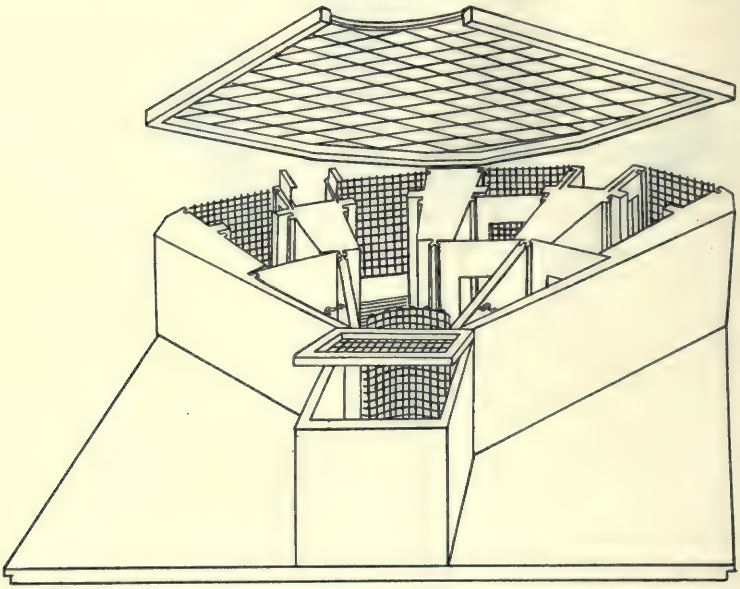


FIGURE 9—Perspective of apparatus for testing habit-formation, ground plan of which is presented in fig. 8.

a no. 6 Columbia dry cell which was connected with the primary coil of a Porter inductorium, the secondary coil of which was in circuit with the ends of the two wires that were wrapped parallel to each other about floors that were fitted into the electric chambers E. During the tests the apparatus was so placed that the entrance box was nearest the source of light and the sides of the apparatus were symmetrically situated with reference to the two windows that admitted the light. The cards used at D were 14.25 x 19.5 cm. and those at F 13.5 x

18.75 cm. In all color work the openings in the two sets of cards were the same in size, 8.5 cm. wide and 11.5 cm. high. These rectangular openings were cut in the lower part of the card in such a way as to leave equal margins of color on either side of the entry way.

III. Method

For a test of the reactions to color, form or size the appropriate cards were placed in the card-holders. For example, if reactions to black and blue were to be tested, one might start with a black card in each of the holders at D and F, right, and two blue cards in the corresponding holders on the left. The cage in which the animals lived was carried to a position with reference to the table upon which the apparatus sat such that one of the wire netting doors of the cage would turn back toward the exit-end of the apparatus as above described, and form an easy passage-way from the reaction box to the cage.

The chick to be experimented upon, marked and numbered, was carried from the cage to the entrance box. When placed in A, it soon acquired the habit of crowding up near the wire netting door, standing at a point equidistant from the two cards exposed at D. It also soon ceased pecking at the wire covering or flying against it, if it happened at first to try this way of escape. The work had not gone very far before the plan was rigidly adhered to of not releasing a chick from A until it made an effort to escape by urging toward the screen. Sometimes this meant a delay of several minutes, but it seemed useless to spend time with a chick that did not react negatively to the confinement or isolation. Further, care had at all times to be exercised that a chick be not kept struggling in A. Results were vitiated if this occurred. I feel sure that discriminating ability deteriorated as the response to the isolation of the reaction box approached too near in one direction the passivity of indifference, or in the other the activity of excitement. I have watched the record of indifferent chicks recover from a lapse when the response to isolation was accentuated by sufficiently prolonged solitary confinement between trials; on the other hand it is easy to point to numerous instances where a record has been marred by undue excitement. I concluded from experience, that much the wisest plan was to watch for an incipient struggle to escape, and to lift the door just when the

impulse had begun to gather force. There is a nice middle ground here that gave the best results. There were great individual differences in the animals. Some stood about for a while before the impulse to escape seized them, others rushed immediately toward the screen door. The experimenter stood back of the entrance box between the two windows of the room, and, after releasing the chick from A, remained as quiet and motionless as possible until the animal found its way out of the apparatus. I had practically no success with the device Yerkes used with the dancing mouse, the card by which he gradually forced the animals toward the electric box. Chicks that won't work without force won't work with it, is a rule that can be generally relied upon. After the animal had escaped to the cage, the experimenter secured it again, brought it back to the entrance box, and the procedure as described above was repeated.

At the beginning of a set of experiments the animal was familiarized with the apparatus, as well as tested for its preference of the stimuli presented in the card-holders, by a number of "preference trials," usually one series, which consisted of ten trials. Sometimes a second series was given. These series are denoted in the tables by A and B, respectively. During the preference trials the exits at H were both left open, the two sets of cards were alternated right and left after each trial, and no electric shock was used. To show the readiness with which the chicks reacted in the preference trials as well as to suggest the availability of the animals for certain kinds of labyrinth training, I present below as typical the average time in seconds for each preference trial, from one to ten, of chicks nos. 32, 33, 38, and 39, Group I, at the beginning of their black-blue work: 69.2, 24.1, 18.1, 3.8, 3.4, 2.6, 2.1, 1.9, 2.2, 1.9. Chicks thus tested for an experiment were continued in that experiment, unless they were found to have a preference for the stimulus to which it was desired they should form the habit of positive reaction. A chick that revealed a preference for this stimulus prior to training was rejected.

After the preference trials had proved a chick satisfactory, it was subjected to the regular conditions of training. The "wrong" side of the apparatus was closed at the exit and the animal was given a shock if it entered the electric chamber on that side. This was done by quietly pressing the key previously referred

to. The shock was not severe enough to cause the chick to jump from the wires or chirp on receiving it. It was found that a shock from a current value of about 1.69 amperes with the secondary coil set at 5.5 according to the scale on the inductorium, proved most generally satisfactory. Here again there were individual differences in sensitiveness among the chicks, but in the main the above shock brought results without being injurious, and had no more effect on the chicks than to start them from their position, occasionally causing one to lift a foot from the wires. The floor of the entrance box was kept wet so that the feet of the chicks would be in a condition to receive the shock with regularity.

At first a chick usually ran right over the wires and on to the front netting, from which there was a full view of the other chicks in the cage. With the chick in this position on the "wrong" side, it was found best to wait until it happened upon the right road out by its own random activity. Some times the wait on the first training trial was ten minutes, but such a delay was exceptional. The young chicks, as a rule, were active and soon acquired the habit of withdrawing from the "wrong" side promptly. The draft on the experimenter's time after the training was well under way, averaged about 1.5 minutes per reaction, including everything. No trouble was experienced with hesitancy on the part of the chicks to return over the electric wires. Of course they were never shocked on the return passage.

In a series of trials the positive card was shifted right and left in this order: r, l, r, l, r, r, l, l, r, l. Thus each card in each series was displayed the same number of times on the right- and left-hand sides. When the chick had made a series of errorless reactions with the above changes of cards, it was tested for another perfect series with a different order and an equal number of rights and lefts. If no errors were made this time, a third series with still a different sequence of changes was given, when, if the chick reacted without error, the process of habit-formation was pronounced completed.

In none of the work with this apparatus was it necessary to starve the chick or even to keep it excessively hungry. It is my opinion that hunger did not play a more important part than the reaction to confinement and solitude. True, it is easy

to show that, as a rule, a chick with a full crop is not so good material for an experiment like this as one not so well fed, but these animals while being experimented upon did not always rush about for food as soon as they had reached the cage.

IV. Experiments

In these experiments on modification thirty-eight chicks were used. The work was done from Dec. 15, 1907 to June 30, 1908. To test the interrelation of habits of response to optical stimuli, the necessity arose of discovering at least two methods of measuring modifiability in this sense mode. It was desirable in the case of any one method that the stimuli bear such a relation to each other that the average chick could complete the training within a reasonable time, and that the time should be as nearly as possible constant for different chicks. The feasibility of the electric shock and satisfactory conditions and devices for training the chicks were determined upon after several hundred preliminary trials in a crude reaction box. The apparatus shown in figures 8 and 9 was constructed on the basis of experience in these preliminary tests, combined with the suggestions afforded by the apparatus of Yerkes [1] and that of Hamilton [2] used in similar experiments. Means for retaining the animal in the entrance box until it made movements to escape, permitting it meanwhile to be within range of the stimuli; the location of these stimuli at a suitable distance from the point of release; the open screen overlooking the cage—these features were planned with characteristic activities of the chicks in mind. Releasing the chick too soon or too late at any given trial produced unsatisfactory results. If the cards were too near, the pulse of activity often carried the animal into the electric box before the card stimuli had wrought the proper effect. And when the animal had made an error and had arrived at the closed exit, the chicks in the cage below acted as an additional stimulus to escape. The animals readily found the exit through H when the door was open.

A. Color

In all the color work the reflection method with the Bradley standard colored papers was employed. Hue, tint, and shade

[1] Yerkes, R. M.: The dancing mouse. New York, 1907, p. 92.

[2] Hamilton, G. V.: A study of trial and error reactions in mammals. *The Journal of Animal Behavior*, 1911, vol. I, p. 33.

are described only by reference to the catalogue name or number of the papers. Measurement of tint (or brightness) was attempted in only a few special cases. The purpose of these experiments at their inception was not to investigate the color reception of chicks, but to test modification of behavior. However, the reactions to reflected colored light proved so interesting that a slight digression from the central interest of the research was later made in the effort to gather some new facts in this closely related field.

Chicks were tried first with Bradley black and white. Most of the first tests involved many elements of irregularity. In some the right and left exchanges of cards were not balanced in the series. In others, chicks had been shocked too severely and either hesitated about entering the electric boxes or balked completely. Again, work was carried over from the preliminary to the new apparatus. The best of these first results are presented below. Chick no. 7, after being given preference trials which were of questionable value, was regularly trained to avoid white and go to black.[1]

TABLE 8

BLACK-WHITE REACTIONS

CHICK NO. 7. HATCHED 12/10, '07. SEX, F.[2]

Series[3]	Date	Right	Wrong
1	Jan. 8	11	9
2	" 10	13	7
3	" 11	13	7
4	" 13	10	10
5	" 14	12	8
6	" 15	9	11
7	" 16	7	13
8	" 17	18	2
9	" "	18	2
10	" 20	17	3
11	" "	7	3
12	" 21	17	3
13	" 22	19	1
14	" 23	17	3
15	" 24	9	1
15	" "	10	0
16	" 25	20	0

[1] In all the tables of results in discrimination work the stimulus to which a chick was forming the habit of positive reaction is each time mentioned first. It is to be assumed that the animals were without previous training unless a definite statement to the contrary is made.

[2] All sex determinations were made by dissection.

[3] In some of the preliminary work the regular 10-trial series was doubled.

In table 9 are presented the results obtained from chick no. 9 under conditions exactly similar to those under which no. 7 was tested. No. 9, however, was trained to accept white and reject black.

TABLE 9

WHITE-BLACK REACTIONS

CHICK NO. 9. HATCHED 12/10, '07. SEX F.

Series	Date	Right	Wrong
1.	Jan. 9	11	9
2.	" 10	9	11
3.	" 13	13	7
4.	" 14	12	8
5.	" 15	13	7
6.	" 16	14	6
7.	" 17	20	0
8.	" "	10	0

Now, if some animals were going to require, as no. 8, 310 tests besides preference trials to complete the black-white training, working with this habit would consume too much of the experimenter's time. An objection to the white-black habit lay in the preference prior to training that chicks showed for the white. If a combination of visual stimuli could be discovered for neither of which the chicks showed any marked preference, such a combination would commend itself for use, provided the time required to form the desired habit of reaction should be fairly uniform among different chicks and conveniently short. The combination, orange and white, was tried with the result shown in table 10. The work of chick no. 12 was devoid of the irregularities spoken of above.

TABLE 10

ORANGE-WHITE REACTIONS

CHICK NO. 12. HATCHED 12/10, '07. SEX, F.

Series	Date	Right	Wrong
A and B	Jan. 29	11	9
1	" 30	6	4
2	" 31	6	4
3	Feb. 1	10	0
4	" 3	9	1
5	" 4	9	1
6	" 5	10	0
7	" 6	10	0
8	" 7	10	0

This record was the cleanest and most promising so far, but was offset by the results obtained from experiments conducted concurrently upon no. 10. The latter chick, in a period of training twice as long as that given to no. 12, failed to acquire a perfect habit of response to orange. The preference trials in each case were satisfactory.

TABLE 11

ORANGE-WHITE REACTIONS

CHICK NO. 10. HATCHED 12/10, '07. SEX, M.

Series	Date	Right	Wrong
A and B	Jan. 29	10	10
1	" 30	5	5
2	" 31	4	6
3	Feb. 1	5	5
4	" 3	4	6
5	" 4	5	5
6	" 5	4	6
7	" 6	4	6
8	" 7	5	5
9	" 8	6	4
10	" 10	9	1
11	" 11	8	2
12	" 12	8	2
13	" 13	9	1
14	" 14	7	3
15	" 15	9	1
16	" 17	9	1

Chick no. 8 was given more searching preference tests and then trained on black-blue.

TABLE 12

BLACK-BLUE REACTIONS

CHICK NO. 8. HATCHED 12/10, '07. SEX, M.

Series	Date	Right	Wrong
A	Jan. 27	Orange 13	Blue 27
B	Jan. 28	Black 6	White 14
C	Jan. 28	Yellow 7	Blue 13
D	Jan. 29	Black 6	Yellow 14

			Black	Blue
1	Jan.	29	2	8
2	"	31	1	9
3	Feb.	1	1	9
4	"	3	2	8
5	"	4	3	7
6	"	5	6	4
7	"	6	4	6
8	"	7	8	2
9	"	8	7	3
10	"	10	8	2
11	"	11	8	2
12	"	12	10	0
13	"	13	10	0
14	"	14	10	0

Preference tests B and D, table 12, indicate a natural preference for the brighter of the two cards. The orange-blue and yellow-blue preference tests brought to light interesting data. The yellow here was very light, much brighter than the comparatively dark blue, not only as judged directly by the human eye, but as tested by the flicker method. Particular attention is called to these first reactions to blue for, in so far as this paper has anything to contribute to the study of color vision, the interest will largely center in and results will turn upon the reactions of the chicks to blue.

The training of no. 8 on the black-blue was now carried through with the result detailed in table 12. Within a reasonable number of trials, 140, including the final three perfect series, the chick nicely developed the habit of selecting the black. It was thereupon decided to adopt the black-blue combination for a more searching test, with a view to its use in the determination of the rate of modification, the permanence of habit, and some of the effects of training.

Chicks nos. 16, 17, 18, 19, and 20 were selected for this work. From all appearances they were normal chicks, and were chosen at random from the healthy specimens in a flock of seventeen. They were twelve days old when given their preference tests. In all tests prior to those reported in table 13 but one set of cards was used, the cards at D, figure 8. In this set of experiments, after series 2, an additional pair of cards was placed at F, whereby shock and color stimuli were presented simultaneously and not successively only, as in the previous experiments. In table 13 the rate of decrease of error or the rapidity of modification is shown in the column, "average number of

errors." On the basis of these results with black-blue there seemed to have been developed one habit of response to optical stimuli that would serve both for a measurement of the rate of modification in the chick and, when used along with another method yet to be discovered, as a test of the interrelation of modifications.

TABLE 13

BLACK-BLUE REACTIONS

CHICKS NOS. 16 TO 20. HATCHED 2/5, '08.

Series	Date	No. 16 Sex, F. R	No. 16 Sex, F. W	No. 17 Sex, F. R	No. 17 Sex, F. W	No. 18 Sex, F. R	No. 18 Sex, F. W	No. 19 Sex, F. R	No. 19 Sex, F. W	No. 20 Sex, M. R	No. 20 Sex, M. W	Av. No. of errors
A	Feb. 17	5	5	5	5	3	7	4	6	5	5	5.6
1	" 18	3	7	3	7	1	9	6	4	4	6	6.6
2	" 19	3	7	3	7	5	5	5	5	6	4	5.6
3	" 20	6	4	7	3	2	8	3	7	6	4	5.2
4	" 21	9	1	8	2	8	2	6	4	8	2	2.2
5	" 22	7	3	10	0	5	5	6	4	8	2	2.8
6	" 24	9	1	9	1	9	1	6	4	9	1	1.6
7	" 25	6	4	10	0	7	3	10	0	10	0	1.4
8	" 26	10	0	10	0	9	1	10	0	10	0	0.2
9	" 27	10	0	10	0	10	0	10	0	10	0	0.
10	" 28	9	1			9	1					0.4
11	" 29	10	0			10	0					0.
12	Mar. 1	10	0			10	0					0.
13	" 2	10	0			10	0					0.

The persistence of habits and their effects, as well as the bearing of the results upon the problem of color vision, will be discussed separately in a later section of the paper. For the present, interest is confined to certain aspects of the process of modification, and the question as to whether the reaction of the chick is determined by the quality as well as the intensity of the color stimulus is not a matter of special concern. The term color is used simply to denote a certain stimulus, with no implication that the chick's reactions are affected by variations in wave-length of the ether vibrations.

Let us turn now to the search for a second method of measuring modification.

B. Form

To test the animals' responses to forms, black cards, cut as previous cards, were used in the regular card-holders of the apparatus. The cards bore two-dimensional forms above the openings through which a chick had to pass on its way out of the appar-

atus. That is, forms were cut out of white paper and pasted midway between the two sides and a little below the top of the card. The chicks were first tested with a square and a triangle of equal area. The triangle had a base of 8.13 cm. and an altitude of 6.35 cm. The square was 5.08 x 5.08 cm. No. 11, a chick that had succeeded in acquiring the black-white habit to the point of making one error in the last twenty trials, reacted in the square-triangle test as shown in table 14.

TABLE 14

REACTIONS TO FORM

CHICK NO. 11. HATCHED 12/10, '07. SEX, F.

Square-Triangle

Series	Date	Right	Wrong
A	Jan. 31	5	5
1	" "	1	9
2	Feb. 1	3	7
3	" 3	4	6
4	" 4	6	4
5	" 5	6	4
6	" 6	4	6
7	" 7	7	3
8	" 8	5	5
9	" 10	2	8
10	" 11	4	6
11	" 12	6	4
12	" 13	2	8
13	" 14	3	7
14	" 15	6	4
15	" 17	4	6

Not only were the results here purely negative, but one had only to watch the chick to become convinced that the probability of its forming the habit of selecting the square was almost nil.

No. 13, a chick that had acquired the white-black (darkened [1]) habit perfectly, was tried at the same time by the same method in this form test with the result shown in table 15.

No. 13 showed no signs of improvement and the experiment was discontinued. The square-triangle combination was discarded for a "design" that was placed at the top of the cards. This time the cards in the apparatus were white and the forms upon them black. On one set of cards were pasted, vertically

[1] The electric chamber on the side of black was covered with cardboard.

and 2.5 cm. apart, two parallel bands of black paper, each band being rectangular in form and measuring 4.2 x .9 cm. On the other set of cards two black bands of equal area and the same material were fastened at right angles to each other and in this wise (Λ). Chick no. 21 was tried first with this combination of forms, but with negative results. No. 15 was tested at the same time. It showed no improvement after 60 trials and from that on the continuous training plan was used. On Mar. 1 it was given 160 trials. The regular system of card exchanges was disregarded and one setting of cards was allowed to remain during several successive trials to see if the chick might thus hit upon the habit of selecting the vertical bars. Sixty trials were given in a similar way on Mar. 2 and as many more on Mar. 3, with negative results.

TABLE 15

REACTIONS TO FORM

CHICK NO. 13. HATCHED 12/10, '07. SEX, F.

Triangle-Square

Series	Date	Right	Wrong
A	Jan. 30	5	5
1	" 31	3	7
2	Feb. 1	4	6
3	" 3	8	2
4	" 4	5	5
5	" 5	6	4
6	" 6	3	7
7	" 7	5	5
8	" 8	4	6
9	" 10	6	4
10	" 11	7	3
11	" 12	2	8
12	" 13	5	5
13	" 14	7	3
14	" 15	6	4
15	" 17	5	5

The plan of having forms at the tops of the display cards was then abandoned and another scheme employed. This time display cards were made of gray cardboard and the forms were presented as openings in the cards through which the chick had to pass on its way out of the apparatus. In one set of cards a circular opening was cut the diameter of which was 8.47 + cm. In the other set of cards a rectangular opening was cut whose base was 6.35 cm. and altitude 8.89. The circle and

the rectangle were constructed as nearly as possible of equal areas. The regular manner of training was resumed and the reactions of chicks 15, 22, and 23 to the circle-rectangle combination were tested. At the risk of being tedious I present the results in detail, for whether or not the ability to react selectively in this case would have demonstrated the dependence of the reactions on the element of form, it did not seem unreasonable to expect the chick to develop a habit of reacting differently to stimuli as different as these.

A glance at tables 16, 17, and 18 reveals only negative results. The chicks soon came to a point in these form tests where they did not make an effort to escape from the box. Precaution was taken to see that the electric shock was not too severe. Previous experience in the color experiments had shown that a chick occasionally received a shock of too great intensity and thereafter hesitated about entering the electric chambers. But the shock for nos. 15 and 23 ran as low as 6 to 6.5 on the scale of the inductorium, and did not reach a greater intensity than 6.5 for no. 22. The regular shock, as stated before, was 5.5. Just prior to the discontinuance of the tests, no. 22 would approach the display cards, hesitate, at times shake its bill, and go no further. The expedients of moving a card over the top of the apparatus from the entrance box toward the

TABLE 16

Reactions to Form

Chick no. 15. Hatched 2/5, '08. Sex, Undetermined

Circle-Square

Series	Date		Right	Wrong
A	Mar.	6	4	6
1	"	"	2	8
2	"	"	2	8
3	"	"	3	7
4	"	"	4	6
5	"	7	4	6
6	"	"	4	6
7	"	"	2	8
8	"	8	2	8
9	"	"	4	6
10	"	"	6	4
11	"	"	5	5
12	"	9	5	5
13	"	"	4	6
14	"	10	3	7

TABLE 17

Reactions to Form

Chick no. 22. Hatched 2/5, '08. Sex, F.

Circle-Square

Series	Date	Right	Wrong
A	Mar. 9	4	6
1	" 10	4	6
2	" "	5	5
3	" 12	5	5
4	" "	5	5
5	" 13	4	6
6	" "	2	8
7	" 16	6	4
8	" "	4	6

TABLE 18

Reactions to Form

Chick no. 23. Hatched 2/5, '08. Sex, Undetermined

Circle-Square

Series	Date	Right	Wrong
A	Mar. 9	4	6
1	" "	9	1
2	" 10	2	8
3	" "	6	4
4	" 12	2	8
5	" 13	6	4
6	" "	6	4
7	" 16	8	2
8	" 17	5	5
9	" 19	4	6
10	" 20	5	5

chick and of forcing him gently from behind were tried without effect. The chick simply "gave up," as my note has it.[1]

C. Size

The search for a second method of measuring modification of behavior was continued in an investigation of the reactions of the chick to size differences in two dimensions, difference in form being excluded. The plan was adopted of having openings in gray display-cards through which the chick had to pass. The openings in one set of cards were 6.35 cm. wide and 8.89 cm. high; those in the other set 8.89 cm. wide and 12.44 cm. high.

[1] Later, during some work as yet unpublished, I found a chick that acquired the circle-square habit when the experiment was conducted in a dark-room. Opal glass was electrically lighted from behind and the above forms, cut in black tin screens, permitted illuminated areas of these shapes to be exposed to the chick.

The openings were the same in shape and one was approximately twice the size of the other. The experiment was undertaken with chicks no. 24 and no. 25, and completed with the result detailed in table 19. The training was finished quite satisfactorily and within a reasonable time limit, so the method seemed available for further use. Five control series were given to check out the influence of brightness. It is manifest that with equal illumination on the faces of the display-cards, which was roughly secured by adjustment of the shades of the two windows that admitted light to the room, the electric box back

TABLE 19

REACTIONS TO SIZE: SMALL-LARGE OPENINGS

CHICKS NOS. 24 AND 25. HATCHED 2/5, '08

Series	Date	No. 24 Sex, M. R	No. 24 Sex, M. W	No. 25 Sex, F. R	No. 25 Sex, F. W
A	Mar. 17	5	5	3	7
1	" 18	0	10	3	7
2	" "	3	7	(frightened?)	
3	" 19	5	5	4	6
4	" "	8	2	6	4
5	" 20	7	3	7	3
6	" "	7	3	5	5
7	" 21	8	2	3	7
8	" "	10	0	6	4
9	" 23	10	0	10	0
10	" "	10	0	10	0
11	" 25			9	1
CONTROL TESTS					
Card of small opening brighter, and its electric box brighter:					
1	Mar. 24	9	1		
Card of large opening brighter, its electric box brighter:					
2	Mar. 24	9	1		
Cards equally illuminated, electric box of small opening made the brighter by illumination of electric light:					
3	Mar. 24	9	1		
Card of small opening brighter, electric box of large opening made much darker by overhead cardboard:					
4	Mar. 24	10	0		
Cards equally illuminated, electric box of large opening made the darker by overhead cardboard:					
5	Mar. 25			10	0

of the large opening would be brighter than that back of the small one. The angle at which the cards stood to each other, together with the position of the windows with reference to the cards, made it possible to vary the brightness of the two cards independently, and that of the electric boxes so that the electric box on the side of the brighter card could be made also the brighter box. A cardboard over an electric box was also used to screen off light. Within the degrees of variation employed, the chicks continued to select the smaller opening regardless of the relative brightness of the faces of the cards or the electric boxes. When both electric chambers were covered with a ground glass plate and a 2 c.p. electric light was adjusted over the electric box on the side of the small opening, no. 24 approached as usual the small opening, hesitated a moment, glanced about the apparatus in the vicinity of the card, and then slowly entered. (See control test 3, table 19.) Control 5 exhibits the same general relations as 3, but without the use of electric light. The pronounced variation, even to a different quality of light, did not inhibit the selective reaction. In control series 4 the electric box on the side of the card having the large opening was darkened considerably by cutting off the light from above with a piece of black cardboard. The figures for this series of reactions, 10–0, in comparison with the 9–1 records in the other series, represent but do not express fully the difference in readiness with which the chick responded. This series repeated control series 1 with greater difference in the relative brightness of the electric boxes. The conclusion seems to be justified, on the basis of these results, that the chicks were responding selectively to one of two objects of different size. Of course the control of other possible determinants of reaction, such as odor, would have made this conclusion more certain. The small-large habit was now adopted to be used in conjunction with the black-blue habit for an experimental test of the effects and interrelation of habits.

D. To What Extent Has Training General Value?

After an experimental test of the influence of one labyrinth habit upon the formation of another, Yerkes [1] concludes that "the acquisition of one form of labyrinth habit may facilitate

[1] Yerkes, R. M.: *Op. cit.*, p. 261.

the acquisition of others." "For the student of animal behavior, as for the human educator, it is of importance to learn," he writes, "whether one kind of training increases the efficiency of similar forms of training." The question for which an answer was sought in the work about to be reported is closely related to the problem which Yerkes set for himself, but is concerned with the more general value of training: How does acquired ability to react perfectly to one kind of element or complex affect the acquisition of ability to react to a quite different element or complex?

Among certain organisms a particular act once acquired is more easily re-acquired. A particular act once acquired may facilitate the acquisition of a similar act, similar meaning here a relation of partial identity. Now the question naturally arises, As two acts are less and less similar, what effect does the acquisition of one have on the later acquisition of the other? This raises the problem of formal discipline, capable, surely, of solution by experiment, but not yet solved. It has been suggested that there may be "two kinds or aspects of organic modification in connection with training; those which constitute the basis of a definite form of motor activity, and those which constitute the bases or dispositions for the acquirement of certain types of behavior." [1] The data upon which this suggestion is based would allow us to say nothing definite about "the acquirement of certain types of behavior" not of the original *labyrinth form*, which Yerkes used. But at that the general value of training has been experimentally demonstrated, in so far as different labyrinths are different objects, even different objects of the same kind. Although there is a degree of generality here, it is so slight that the training value involved would no doubt ordinarily be classified as specific. If no two particular stimuli are identical, then, practically speaking, much specific training has general value, which does not mean, however, that this generality of value may not rest at bottom on specific modifications. This must be our conclusion when we look at the matter from the side of content as opposed to function, stimulus as opposed to reaction. Practically, it cannot be denied that in some organisms certain kinds of training do have value for certain other kinds. The question awaiting answer in the

[1] Yerkes, R. M.: *Op. cit.*, p. 261.

present state of our knowledge is, Between what kinds of modifications and to what degree is there transfer? Few will dispute the assertion that there may be dispositions preceding and underlying the acquisition of a particular act which are not identical with those that are at the basis of its actual performance. Indeed the former may no doubt be congenitally present, with or without the actual ability to perform. In a similar manner, of course, one may think of an acquired neurological disposition "predisposing" the individual to certain new activities. And within what range? That is our problem.

To bring a particular case to a test, two comparable groups of four chicks each were chosen, Group I consisting of chicks nos. 32, 33, 38, and 39, Group II of chicks nos. 34, 35, 36, and 40. All the chicks were of the same brood, hatched April 13, 1908. None of them had any other training prior to or during the course of these experiments. Uniformity in method was strictly adhered to. The chicks of Group I were given the black-blue training to the point where they made no errors for three successive days, ten trials per day. Then the chicks of both groups entered together into the size training. During the course of the development of each habit one setting of the cards was used for the corresponding trial of all the animals. For example, in the black-blue work, the black card occupied the positions r, l, r, l, r, r, l, l, r, l, in each series until an errorless series was reached. At the first setting of the cards—black right, blue left—the chicks of Group I went through one at a time until each animal had to its credit one reaction. Then the cards were exchanged to the second position and the chicks again taken as before.

As a precaution which seemed advisable, the secondary coil of the inductorium was adjusted at 6 to begin with in the black-blue training, and it was continued thus throughout. In the later size training a shock of the same intensity was employed at the start and at the beginning of the ninth series this was raised to 5.5 on all the chicks alike.

The order of training was this. When the chicks of Group I were twelve days old, work was begun with them, and they were trained until they responded errorlessly with a positive reaction to black. On May 11, by which time this group had finished the color work, these animals with those of Group II

were introduced into the size training. Table 20 shows the course of modification of response to the color stimuli. Only the errors appear in this summary.

TABLE 20

ERRORS IN BLACK-BLUE REACTIONS

CHICKS NOS. 32, 33, 38, AND 39. HATCHED 4/13, '08

Group I		32 Sex, F.	33 Sex, M.	38 Sex, M.	39 Sex, F.
Series	Date	W	W	W	W
A	Apr. 25	5	5	5	5
1	" 26	6	7	7	7
2	" 27	3	3	5	4
3	" 28	1	3	3	2
4	" 29	0	3	2	4
5	" 30	0	2	1	2
6	May 1	0	1	2	0
7	" 2		1	1	1
8	" 3		0	1	0
9	" 4		0	0	0
10	" 5		0	3	0
11	" 6			0	
12	" 7			0	
13	" 8			0	

These chicks really do not vary so much in the modifiability of their behavior as the figures might indicate. No. 38 became excited during the tenth series, which may explain its lapse. The rapidity with which no. 32 finished the work is noticeable. No chick before or afterwards in my experiments excelled this record of 30 trials exclusive of preference and perfect series. But it is sufficient for our present purpose simply to show clearly the perfection of the training that preceded the formation of the second habit, without extended discussion of the other characteristics of the modification.

Following the completion of this training of Group I, both groups were introduced into a size experiment, as described above. Table 21 reveals the progressive development of the second modification for each group in the column of average daily number of errors. The total of these averages for Group I amounts to 66.3 for the sixteen days training; for Group II, 70.1 during the same period. Curves I and II, fig. 10, exhibit graphically the rate of decrease of error for the corresponding groups. The record of each group in the preference tests is

included in the curves. In a comparison of results for the two groups two things especially must be taken into consideration, (1) the natural preferences before training began and (2) previous training. In regard to the first, Group II had to overcome a stronger preference for the larger of the two openings. The chicks of Group I took the smaller opening more often than those of Group II, perhaps because in general the black-blue

TABLE 21

RECORD OF ERRORS

REACTIONS TO SIZE: SMALL-LARGE OPENINGS

HATCHED 4/13, '08

Series	Date	Group I					Group II				
		32	33	38	39	Av.	34	35	36	40	Av.
A..........	May 11	6	7	5	3	5.3	5	5	7	8	6.3
1...........	" 12	8	5	9	7	7.3	8	9	5	9	7.8
2...........	" 13	6	7	6	7	6.5	6	9	10	5	7.5
3...........	" 14	6	5	6	8	6.3	6	7	5	7	6.3
4...........	" 15	7	7	4	7	6.3	5	5	7	7	6.
5...........	" 16	5	5	5	7	5.5	7	8	6	7	7.
6...........	" 18	5	4	6	3	4.5	6	6	4	6	5.5
7...........	" 19	7	7	2	1	4.3	3	4	3	6	4.
8...........	" 20	5	8	2	3	4.5	2	6	1	5	3.5
9...........	" 21	3	5	4	2	3.5	5	4	1	8	4.5
10...........	" 22	4	7	3	2	4.	3	3	3	6	3.8
11...........	" 23	6	3	2	2	3.3	1	2	2	6	2.8
12...........	" 25	4	6	2	4	4.	2	3	0	4	2.3
13...........	" 26	1	1	1	1	1.	1	2	4	5	3.
14...........	" 27	1	2	1	2	1.5	1	2	3	4	2.5
15...........	" 28	0	1	4	1	1.5	1	1	1	4	1.8
16...........	" 29	2	1	5	1	2.3	1	0	1	5	1.8
		Total av......66.3					Total av......70.1				

training inclined chicks to accept the stimulus of lower brightness value, which in this case has been shown to be on the side of the smaller opening. The previous brightness training here came into conflict, however, with the natural(?) tendency to pass out by the larger opening, which may be something more than a brightness preference, and thus may be explained the small difference between the two groups in the results of the preference trials. As to the second point above, Group I, besides acquiring the black-blue habit, had become habituated to the general conditions of the apparatus in such a way that adjustment to the new conditions of the size experiment must have

been somewhat facilitated—through a lessening of the tendency to general excitement during the first tests, if in no other way. In view of these considerations there is no conclusive evidence that the previous training of Group I with black-blue facilitated the formation of the small-large habit, albeit Group I averaged 5.4% less errors per day than Group II.

It should be stated in connection with the above size work that the results indicate difficulty of acquisition. The behavior of the chicks pointed in the same direction. So, any more general conclusion than we have ventured would have to be made sub-

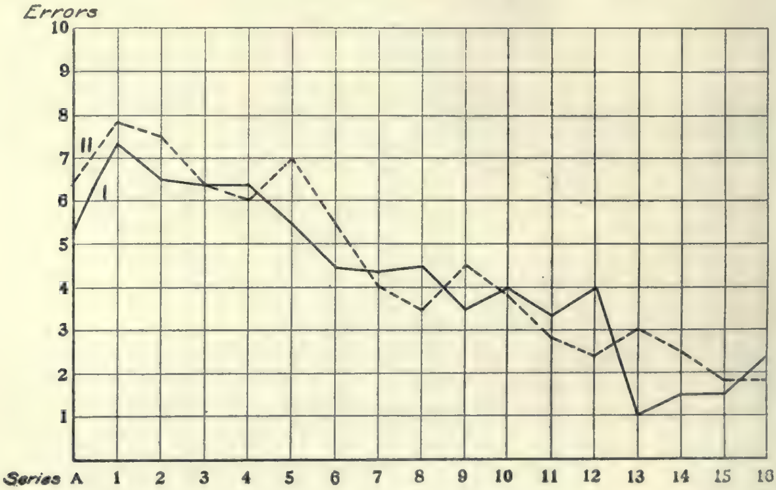


FIGURE 10—Rate of modification in size training, arranged to test the effect of previous color training. Curve I shows the rate of acquisition by Group I of the small-large habit after having acquired the black-blue habit; Curve II represents the rate of acquisition of the same habit by Group II, the same number and the same age as the chicks in Group I, but without previous color training. Preference tests on first day. Distances along axis of abscissae represent series; along axis of ordinates, average number of errors.

ject to this further fact. It ought also to be pointed out that chicks of the same age are not always of the same size. The members within each group were of unequal size when six weeks old. Yet, fortunately for our experiment, the groups remained comparable as to size, individual for individual, throughout the tests. This matter is vital to the experiment, for naturally chicks of different sizes could pass through the small opening with different degrees of ease.

But the conclusion as to the general efficiency of the black-blue training should not rest alone upon a comparison of the rates of modification in the two groups of chicks. Having learned in the course of the experimentation with the black-blue habit, from data which will appear later, that this modification persists for some time with relatively slight change, it seemed of value to investigate the relation of the size training to the color training by means of the so-called memory test.

TABLE 22

THE NON-INTERFERENCE OF MODIFICATIONS

Training in Black-Blue					Training in Small-Large			Persistence of the Black-Blue Modification			
No.	Age	Sex	Trials	Errors	Age	Trials	Errors	Interval	R	W	First selection
32	12	F	30	10	28	160	70	32	10	0	Black
33	12	M	70	20	28	160	74	30	10	0	Black
38	12	M	100	25	28	160	62	31	4	6	Blue
39	12	F	70	20	28	160	58	30	9	1	Blue

Table 22 exhibits the results of this test. The table shows the age of the chicks of Group I when the work on black-blue was begun, 12 days; the number of trials necessary to complete the training, exclusive of preference and final perfect series; the number of errors made in the course of this training; and also the corresponding age, trials and errors for the size training. The interval, thirty days or more in each case, is the number of days between the date when the black-blue modification was pronounced perfect and the date upon which the memory test was given. Of course sixteen days of this interval were occupied with the development of the small-large habit.

The reactions of the chicks to black-blue in the persistence tests are detailed in the columns R and W. Chicks nos. 32 and 33 made perfect records, no. 39 made an error on its first trial, being thereafter perfect, while the reactions of no. 38 seemed to show no trace of the original modification. The results altogether weigh in favor of the view that the modifications with which we have dealt proceed quite independently

of each other. By consulting table 26 it will be seen that the persistence of the black-blue habit in these chicks after 30 days measures fully up to the average made by other chicks that had not had intervening training. If it be objected at this point that one should expect the small-large training to exert an influence in the direction of strengthening the black-blue modification, I agree, in so far as brightness is concerned, but the inference is that something more than brightness was involved in each habit. That color and not brightness was the effective element in these tests of persistence I have not shown. The results are offered in their present stage for their possible value as a suggestion for method. It is not too much to expect relative permanency of the black-blue habit even if the brightness relations of the first training were reversed in the second. Though both forms of training are habits of response to optical stimuli, they are probably quite separate from each other in their development. Physiologically, this likely means the formation of separate neural bases. For our explanations we look to the central nervous system. Judd[1] observes that "Discrimination is not a process of impression." "The raw materials for adaptation, the impressions, are there very early, waiting for the individual gradually to adjust himself to them." Discrimination depends upon the ability of the individual to react specifically. Capacity to discriminate is linked with the capacity to react. If this be true, the animal's scope of reaction will determine the limits of its modifiability. According to this view, the grouping of elements to form a single object of reference in reaction derives its unity from the co-ordination in a single active brain process. This does not ignore the fact that the structure of the end organs will also define in advance the range of objects that may be discriminated. Impressions to be distinct must be accompanied by distinctive neurological dispositions. "In short," says Judd again, "so far as this raw material for development which presents itself in the form of impressions is concerned, it must all be worked over and connected with individual reactions before it can be regarded as really assimilated by the developing individual."

But let us return from theory to facts.

[1] Judd, C. H.: Genetic psychology for teachers. New York, 1903, p. 153 ff.

E. Some Effects of Modifications

In table 12 were pointed out the natural color preferences of chick no. 8: white in preference to black, blue to orange, blue to yellow, and yellow to black. The peculiarity of the reactions to blue was mentioned. If blue be supposed to have a higher brightness value for chicks than for humans, the chick's behavior might be satisfactorily explained on the ground only of brightness of stimulus. Further experimentation was undertaken with a view to an explanation of the above apparent irregularity. No. 11, a chick which had been trained on black-white and had progressed so far that in its last series of twenty successive trials it made but one error, was tested for its preference on the combination of colors listed in table 23. Eight days had elapsed between the date of the 19–1 series and these color preference tests.

TABLE 23

PREFERENCE TESTS AFTER BLACK-WHITE TRAINING

CHICK NO. 11. HATCHED 12/10, '07. SEX, F.

Series	Date	Right	Wrong
	Jan. 21	Black 19	White 1
A	Jan. 29	Orange 8	White 2
B	Jan. 29	Black 10	Blue 0
C	Jan. 29	Blue 6	White 4
D	Jan. 29	Yellow 8	White 2

This record reports the tests in the order in which they were given, as do other records. Again the reactions to blue seemed somewhat surprising. That blue should be rejected as freely as white, when appearing with black, and that blue and white should be accepted almost indifferently, are not results that one should expect. These facts seem to confirm the hypothesis that blue has a special brightness value for the chick.

Further investigation followed with six other chicks, incidental to the direct line of investigation. Table 24 shows the first preference reactions of chicks nos. 17, 18, 20, 32, 33, and

39 to black-blue. The preference tests are not representative of our chicks in general, for those chicks that already had a preference for black were rejected because they were not suitable for the special purposes of the experiment of which this black-blue training formed a part. There were a few of these. But this does not affect in the least the point that I wish to make in connection with the effects of training. The state in which the black-blue modification persisted at the time of the later color preference tests is shown in the columns under "Persistence." The tests on blue-white, black-white, blue-yellow,

TABLE 24

Some Effects of Training

No. of chick	Age	Training	Preference		Interval	Persistence		Interval	Preference after Training										
			Black	Blue		Black	Blue		Blue	White	Black	White	Blue	Yellow	Interval	Blue (tint no. 1)	Yellow	Blue	Blue (tint no. 1)
17	12	Black-Blue	5	5	30	10	0	2	4	6	10	0	3	7					
18	12	"	3	7	30	8	2	0	3	7	8	2	2	8					
20	12	"	5	5	30	10	0	2	3	7	9	1	1	9					
32	12	"	5	5	32	10	0	0	0	10	3	7	0	10	3	1	9	1	9
33	12	"	5	5	30	10	0	0	0	10	6	4	0	10	1	0	10	5	5
39	12	"	5	5	30	9	1	0	2	8	5	5	1	9	1	5	5		
49	39	None	1	9											1	10	0		
51	39	"	1	9											1	7	3		

blue (tint no. 1) -yellow, blue-blue (tint no. 1) were given in the order named and, in all but two cases, immediately following the persistence tests. The most striking feature of these results is again the character of the reactions to blue. If brightness were the only factor, a chick trained to black-blue would be expected thereafter to react positively to the less bright of two stimuli, other things being equal, when a different combination were presented. But chicks that one minute accepted black and rejected blue, that is, selected the card of lower brightness value, the next minute accepted white and rejected blue. How shall we explain this behavior if we consider merely the

brightness values of the stimuli? After black-blue training the chicks quite commonly rejected this blue no matter with what other color it was in combination. It may be suggested that after the long period of training the chicks respond to a particular brightness value, the blue amounting to a certain degree of gray. But no. 32 and no. 33 rejected blue (tint no. 1) when it was used in combination with the much brighter yellow.

The method of training with the electric shock frequently makes the stimulus to which it is attached the emphatic one. In a word, the chicks thus trained may not be guided in their reactions by the color through which they escape, but by the color in connection with which they are shocked. This reaction is one primarily of rejecting blue, rather than of accepting black. It is indicated by such reactions as those of nos. 32, 33, and 39 to black-white after having been trained to black-blue. When white replaced the blue of the original training combination, the stimuli became almost indifferent to the chicks. Again, the same chicks seemed indifferent to the color presented with blue. When required to react to blue-white there was no hesitancy, the response being one of getting-away-from-the-blue-no-matter-what-the-other-color. For data see table 24.

All the chicks did not behave so, however. A comparison of the corresponding reactions of nos. 17, 18, and 20 is instructive. These animals, when white replaced blue as above, responded definitely and positively to black, and were quite lost when white accompanied blue. With these chicks black seemed to be the guiding stimulus.

It is sometimes said that negation is affirmation, but is this true in any proper psychological sense? Naturally, avoidance of one object means the acceptance of some other object—in the sense that leaving one place means going somewhere else. But does the "somewhere else" need to be defined as a constituent element of the avoiding reaction? Or, upon the occurrence of a positive response, is it necessary to consider the point of departure, which is objectively definable, of course, as a part of the process? The point of departure in the latter or of arrival in the former case does not appear to be essential.

Passing now from the standpoint of stimulus to that of reaction, why should a positive response be regarded as a simultaneous negative response? As antagonists may they not mutu-

ally exclude each other? But only in so far as a positive reaction essentially includes a negative one and *vice versa*, *is* affirmation psychologically negation.

There still remains another aspect of the relation between rejecting and accepting, their possible necessary concomitance. But if, with innervation of a given motor center, there is, according to the Muensterbergian action theory, a simultaneous inhibition of the antagonistic motor center, we are bound to think of real affirmation and real negation as distinct functions, for inhibition is not negation.

What, then, shall we say of our method as a "discrimination method?" From the point of view of behavior discrimination is selective reaction. In this sense the Yerkes method is a discrimination method.

It is interesting to observe the result when neither black nor blue are in the test combination presented after the training. No. 44 on May 22 completed the black-blue habit with three errorless series. On June 6 its persistence test was black, 10; blue, 0. The same day its preference tests on orange-white resulted 5–5. The daily training on orange-white, continued from this date, progressed as follows: 7–3, 8–2, 7–3, 10–0, 10–0, 10–0. No. 43, the same age, began black-blue training on the same date as no. 44 and completed the work the day after no. 44 with three perfect series. Its persistence test on June 6 resulted also 10–0. But when tested on the same day for its orange-white preference prior to the training on this combination, its record was orange, 10, white, 0. The figures thereafter were 9–1, 9–1, 9–1, 10–0, 10–0, 10–0. It is apparent in both these cases that the black-blue training strikingly predisposed the chicks to react positively to the darker of the two stimuli in the second combination. It cannot be urged in regard to no. 44 that, either on account of ready modifiability or an easy combination, this was simply a case of rapid learning unassisted by the previous acquisition, first, because orange-white, as indicated by earlier tests, is not so easy a combination; and secondly, the rate of acquisition by this chick was only average, nine days' training having been required for it to complete the black-blue habit.

The important exception to the general rule for brightness training, namely, that when the color which the chick was

trained to avoid was presented in combination with a new color, the chick continued its specific avoiding reaction regardless of the relative brightness values of the stimuli, demands, I believe, that color quality as well as intensity be assumed as a determining factor in the reactions of these chicks.

F. The Persistence of Modifications

Hints of the material about to be presented have necessarily been dropped in the discussion of previous topics. Yerkes [1] publishes in his work on the dancing mouse a valuable table of results on "Measurements of the Duration of a Habit." Commenting on this data, he says, "It is safe, therefore, to conclude from the results which have been obtained that a white-black or black-white discrimination habit may persist during an interval of from two to eight weeks of disuse, but that such a habit is seldom perfect after more than four weeks." If one should assume that the chicks depended upon the element of brightness only, the results obtained for them in our black-blue tests would be comparable with the black-white and white-black records of the mice, provided one allowed for age differences, as well as for the fact that the discrimination, assuming the human standard, was made more difficult for the chicks on account of the smaller difference in the brightness of the two stimuli.

The chicks in these tests (see table 25) were placed in the entrance box in the manner followed in the original experiments and allowed to escape from the apparatus to the cage. The cards were exchanged after each trial, that is, regularly alternated right and left. No shock was given and both exits remained open throughout. There was a noticeable difference in the behavior of the chicks in the apparatus after the interval. Instead of smoothly going to work they usually hesitated a few moments and craned their necks about before starting for an opening. The same kind of phenomenon was noticeable, ordinarily, in the behavior of a previously trained chick when lifted in the hand after an interval of some weeks during which it had not been handled. Whereas the animal had come to submit freely and without struggle in the course of the original training, now, when grasped over the back, it sometimes kicked,

[1] Yerkes, R. M.: *Op. cit.*, p. 256.

chirped, or attempted to make flapping movements with the wings. For the tests of persistence, excitement of this kind was avoided as much as possible.

In estimating the value of the results printed in table 25, it must be remembered that chicks nos. 32, 33, 38, and 39 had 16 days' size training in the interval. Also, nos. 43 and 44 had orange-white training to perfection between the first and the second persistence tests.

TABLE 25

The Persistence of Habit

Chick	Sex	Training	Days' interval	R	W	First selection
16	Female	Black-Blue	30	10	0	
17	"	"	30	10	0	
18	"	"	30	8	2	Black
19	"	"	30	7	3	"
20	Male	"	30	10	0	
24	"	Small-Large	19	6	4	Large
25	Female	"	17	6	4	Small
32	"	Black-Blue	32	10	0	
"	"	"	55	8	2	Black
33	Male	"	30	10	0	
"	"	"	51	9	1	"
34	"	Small-Large	71	9	1	Small
38	"	Black-Blue	31	4	6	Blue
"	"	"	48	5	5	Black
39	Female	"	30	9	1	Blue
43	"	"	14	10	0	
"	"	"	21	10	0	
"	"	"	77	10	0	
44	Male	"	15	10	0	
"	"	"	22	10	0	

Of eight mice which Yerkes trained in black-white or white-black, only two showed perfect persistence of the habit after an interval of four weeks. Of nine chicks trained in black-blue and tested in a similar way after 30 days, five obtained perfect records.

The behavior of no. 34 in the reaction box equipped with the size cards 71 days after the completion of the original training furnished a nice commentary on the "intelligence" of the animal. This chick, standing naturally, was 19 cm. high, measured to the highest point of its back. So large that the larger of the two openings would permit its passing out with difficulty, it crouched at the smaller opening and attempted to go through it at every trial but the last. Each time, as soon as it got its head through the small opening and was just beginning to

struggle, it was brought back to the entrance box for the next trial. The record of its behavior found in table 25 is made subject to the irregularity reported below. The result of this persistence test is the same as that in its last training series. It would be difficult to reach any trustworthy conclusion in regard to the persistence of the small-large habit from the data at hand, for these data have to be considered together with a certain defect in the adjustment of the apparatus. When the persistence tests of nos. 24 and 25 were made, these chicks were so large that they had to *squeeze* through the smaller opening. For the test of no. 34 the small opening was made slightly larger and a dimension of the larger opening altered to preserve similarity of shape.

TABLE 26

THE RAPIDITY OF MODIFICATION AND ITS PERMANENCE

Black-Blue Training

Chick	Age	Sex	Trials	Errors	Interval	Persistence R	Persistence W
16	12	F	100	28	30	10	0
17	12	F	60	20	30	10	0
18	12	F	100	35	30	8	2
19	12	F	60	28	30	7	3
20	12	M	60	19	30	10	0
32	12	F	30	10	32	10	0
33	12	M	70	20	30	10	0
38	12	M	100	25	31	4	6
39	12	F	70	20	30	9	1
Index of modifiability						72.2	

For the assistance it may be to others in estimating the value of these tests of the persistence of modifications, it should be stated that some of the chicks were in poor physical condition when the tests were made. In the confinement of the laboratory few chicks remained in normal physical condition after eight to twelve weeks.

It is interesting to study the above data on the persistence of habit in connection with the number of trials and errors in the original training series to see if any correlation exists between the rapidity of acquisition and its permanence or retention. In table 26 the necessary items are set side by side. One often hears the pedagogical byword, "Come easy, go easy." This

certainly does not apply to the chicks tested. Of three animals that required 100 trials to perfect the black-blue modification, one made a perfect persistence test; of two that required 70 trials, one made a perfect record; of three that required 60 trials, two made perfect tests; and the chick that completed its work in the shortest time also made a perfect record. The number of trials given in the table is exclusive of preference and final perfect series. In the error column is given for each chick the total number of errors in this number of trials. Of five chicks that made twenty or less errors, four had perfect persistence

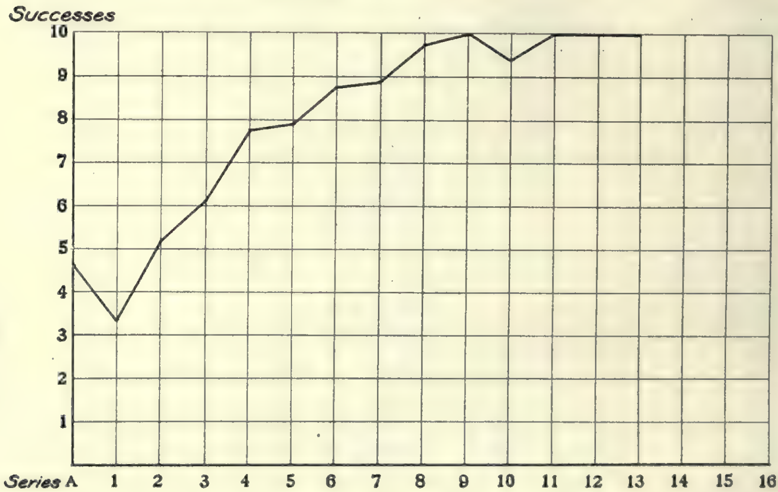


FIGURE 11—The success curve for nine chicks of the same age in black-blue training, plotted from averages based on data given in tables 13 and 20. Distances along axis of abscissae represent series; along axis of ordinates, right or successful trials.

tests; of four that made more than twenty errors, one had a perfect persistence test.

One may well question the appropriateness of the expression "trial and error" intended to be descriptive of this method of learning. If the number of errors is included in the number of trials, as for example in this paper, then the original term is awkward if not illogical. These animals learn through successes as well as failures, and the sum of the number of both equals the whole number of trials. In this sense the method is more accurately described as a method of success and failure. Again,

if the word " trial " in the above expression is meant to refer to the principle of random activity, and " error " to learning based on the avoidance of harmful stimuli, still " trial and error " is an inadequate descriptive term. One could as well say the general law is, " Prove all things; hold fast that which is good." If the term selection be construed broadly enough to include both the positive and negative aspects, then trial and selection appears on the whole to be a more satisfactory name for this type of modification.

If the " index of modifiability," to borrow Yerkes' term and definition, be considered " that number of tests after which no errors occur for at least thirty tests," the index of modifiability of these Barred Plymouth Rock chicks, under the conditions described, is 72.2.

The rate of modification for the same nine chicks is set forth graphically in fig. 11. This is a success curve plotted from data in tables 13 and 20. The averages upon which the curve is based, including the preference test, are as follows: 4.7, 3.3, 5.2, 6.1, 7.8, 7.9, 8.8, 8.9, 9.8, 10, 9.4, 10, 10, 10. It is quite possible that valuable accretions to our knowledge of habit and instinct may come through a careful study of the course of development of reactions of both types and a comparison of the quantitative relations involved. I offer the curve of the development of the complete pecking co-ordination (fig. 7) and that of the development of the black-blue modification (fig. 11) as suggestions for the beginning of a more specialized investigation.

SUMMARY AND CONCLUSIONS.

The early post-embryonic life of the chicks continued the scope of activities already begun in the egg. The alternations of passivity and activity, the lifting movements of the head combined with stretching movements of the legs, the occasional reflex forward thrust of the bill followed usually by movements of the mandibles, loud chirping along with other violent activity, —all these were common aspects of the behavior of chicks just before as well as immediately after hatching. The pecking reaction might have assisted in the process of exclusion, but by far the most common reaction while the chick was struggling in the egg was the lifting movement of the head and bill. This reaction actually broke shell and tore confining membranes.

The chicks, left to develop naturally in the vicinity of food and water, usually found the water by fortuitous pecking or by performing the drinking movement in imitation of other chicks. By imitation here I mean that the performance of drinking by one chick in the presence of another sometimes stimulated that other itself to perform the drinking act. I reach this conclusion on the basis of incidental observations in the course of the study of other problems. I have no doubt that a special study of this point will bear out the above conclusion. Chicks kept without drink until the third day did not perform the drinking reaction by accident or imitation only. The reaction was elicited by a variety of objects before drink of any kind had touched the bill of the chicks, showing that, when the need of the organism became sufficiently imperative, the drinking reaction appeared in response to many of the same kind of stimuli as the unpracticed pecking reaction. The drinking instinct does not "have to be supplemented by imitation, accident, intelligence, instruction, etc., in order to act." And it is not improbable that the "sight of still water" will be found to evoke this reaction.

The few tests that were made to determine the effect on the pecking reaction of disuse during the first two days of the life of the chick seemed to show (1) that the development of the instinct was retarded by disuse, and (2) that the retardation was quickly overcome with use.

The stimulus for the pecking reaction did not have to be some object of a size convenient for eating. The bill of the chick was used as a testing organ.

Pecking situations were found in which the reaction to one stimulus exerted an inhibitory effect on the reaction to another.

The assumption that chicks do not peck at or eat food when they cannot see it is not supported by the experimental data submitted in this paper. The chicks did peck at and eat food when the light was excluded.

The pecking instinct was investigated primarily to discover the meaning that should attach to such terms as perfection, accuracy, or congenital definiteness, when applied to this response. Pecking is a co-ordination of three reactions,—striking, seizing, and swallowing. The amount of improvement from day to day of the complete co-ordination, not only, but of the com-

ponents separately, was measured. Records were taken up to the twenty-fifth day. The pecking improved in accuracy very rapidly during the first two days, reaching by the beginning of the third day an efficiency numerically represented by 29.29 on a scale of 50; by the beginning of the eleventh day an efficiency of 40.10 was attained; and during the rest of the period of measurement the degree of accuracy ran no higher than 42.57. The improvement was retarded more by errors in seizing than by errors in swallowing, and more by errors in swallowing than by errors in striking. After the third day the imperfection of seizure remained greater than that of the other two reactions combined. The striking reaction, seldom widely erroneous, improved rapidly and without relapse, closely approximating perfection by the fifth day,—a degree of accuracy that might easily have inspired belief in the perfection of the pecking instinct.

It did not appear that the effect of social influence was such as to increase the rate of improvement in accuracy of the pecking reaction. It may be that the change in the mode of functioning of the organism due to the presence of others was in the direction of increasing the intensity and rapidity of the reactions, without increasing their rate of improvement in accuracy. Such a variation would have selective value, inasmuch as the animal would get more food in a given time, even though the pecking were no more accurate.

The important exception to the general rule for brightness training, namely, that when the color which the chick was trained to avoid was presented in combination with a color other than that used in the original training, the chick continued its specific avoiding reaction regardless of the relative brightness values of the stimuli, demands, I believe, that color quality as well as intensity be assumed as a determining factor in the reactions of the chicks.

The chicks, without much doubt, responded selectively to one of two objects of different size.

The results of the form tests reported were purely negative.

There was no conclusive evidence that previous formation of the black-blue habit facilitated the formation of the small-large habit.

The tests of retention of the black-blue habit after size training, which intervened between the original training and the retention tests, at least suggest a method that may be useful in studies of the interrelation of habits.

The method of training with the electric shock frequently made the stimulus to which it was attached the emphatic one. In such a case the chicks were not guided in their reactions by the color through which they escaped, but by the color in connection with which they were shocked. That is, the reaction was negative to blue, not positive to black, when this occurred in black-blue training. Psychologically, it seems, negation is not affirmation.

Of nine chicks perfectly trained in black-blue, five made perfect persistence tests after an interval of thirty days.

In the case of the same tests of retention, rapidity of modification was positively correlated with permanence of modification.

For these nine chicks the "index of modifiability" was 72.2.

"Trial and error" is an unsatisfactory name for this method of learning.

No. 12 111/

THE RELATION OF STRENGTH OF STIMULUS TO RATE OF LEARNING IN THE CHICK

BY LAWRENCE W. COLE, (THE UNIVERSITY OF COLORADO)

From the Harvard Psychological Laboratory

ONE FIGURE

The experiments described in this paper were undertaken in order to learn under what strength of stimulus chicks most rapidly learn to make, respectively, an easy, a medium, and a difficult discrimination. Yerkes and Dodson discovered, in the case of the dancing mouse, that when "discrimination is extremely difficult the rapidity of learning at first rapidly increases as the strength of the stimulus is increased from the threshold, but, beyond an intensity of stimulation which is soon reached, it begins to decrease," while when "discrimination is easy, the rapidity of learning increases as the strength of the electrical stimulus is increased from the threshold of stimulation to the point of harmful intensity."[1] In other words, there appears to be an optimal strength of stimulus for each degree of difficulty of discrimination and the intensity of this optimal stimulus is less the more difficult the discrimination which is to be made.

It was proposed, then, to test the chick's rate of learning to discriminate by a method similar to that which had been employed with the dancing mouse. The work was done in the Harvard Psychological Laboratory and my thanks are due to Professor R. M. Yerkes for the plan of the investigation. The method of measuring the units of electrical stimulation and of calibrating the inductorium for that purpose is that of Doctor E. G. Martin of the Harvard Medical School.[2] The values of stimuli are relative, not absolute. Since the publication of the paper of Yerkes and Dodson, referred to above, Doctor Martin has discovered that certain corrections should be made which were not made for the original calibration published in the Yerkes and Dodson paper. All of the values of stimuli used in

[1] Yerkes, Robert M. and Dodson, John D. The relation of strength of stimulus to rapidity of habit formation. *Jour. of Comp. Neur. and Psych.*, 1908, vol. 18, pp. 459–482.

[2] Martin, E. G. A quantitative study of faradic stimulation. I. The variable factors involved. *Amer. Jour. of Physiol.*, vol. 22, pp. 61–74. II. The calibration of the inductorium for break shocks. *Ibid.*, pp. 116–132.

their investigation, as given in their paper, are relative as are those of the present paper.

The chicks. In the experiments sixty-eight barred Plymouth Rock chicks were used, six in preliminary tests and sixty-two under the established conditions of the experiments. The eggs from which the chicks were hatched were all obtained from a single poultry breeder and were guaranteed to be of pure stock. It was necessary, however, to purchase six young chicks of another breeder, but these also were warranted to be pure barred Plymouth Rock chicks and they were kept until it was certain that they presented no marks of difference from the rest of our chicks. Six chicks were used in every series of tests except three. Under the medium condition of discrimination with the weakest stimulus which was employed four chicks were used, while in each of two other groups a chick became sick during the progress of the experiments.

When the chicks were eight days old they were given two days of preliminary training (twenty trials) in order that they might learn the way through the experiment box. This was followed by twenty trials in order to ascertain whether the chick had a preference for either the lighter or the darker screen, thus *the training series began in every case on the twelfth day after hatching.* The training continued until the chick *had made twenty consecutive choices of the darker screen.* Thus the order of tests, for each chick, was (1) preliminary series, (2) preference series, (3) training series.

Apparatus. Figure 1 represents in its essential details the apparatus which was used in the investigation. The electrical connections are omitted and the electric key, K, was somewhat further to the right than appears in the figure. For convenience of description we may consider the apparatus as composed of three divisions or boxes. (1) The hover box, O; (2) the illumination box which contains the electric lamps and has for its nearer end the two opal glass screens N_2 and N_3, and their frame or holder; (3) the experiment box which has the screens and holder for its remote end and consists of two compartments, A and C.

The hover box, O, had dimensions of 100 x 27.5 x 21 cm.[3] Its floor was covered with sand and midway of its length was

[3] All dimensions are given in the order length, width, and depth, and are inside measurements.

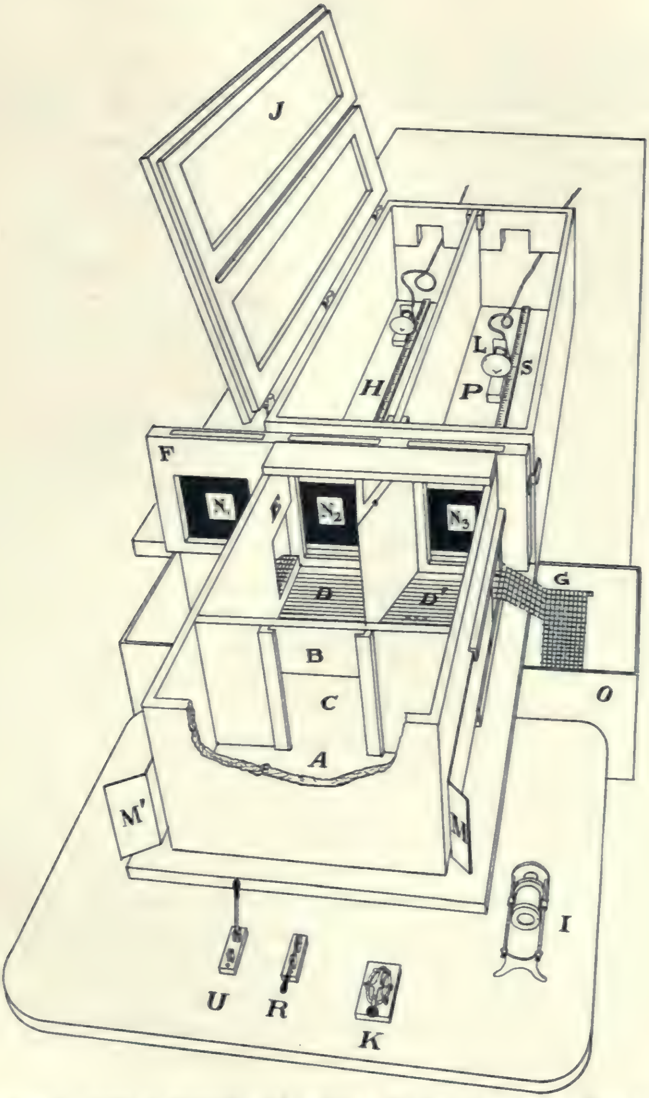


FIGURE 1—The figure is symmetrical, hence the letters G, E, L, and S must be understood to designate both the parts which they respectively mark and also duplicates of these parts on the opposite side of the figure.

1. *Hover box:* O, hover box; G, inclined planes (which were replaced by wooden platforms) of mesh wire leading from the doors, E, of the experiment box to the hover box.

2. *Illumination box:* H, left compartment; P, right compartment; L, lamps; S, metric scales.

3. *Experiment box:* A, compartment in which chick was placed; C, compartment in which it made choice of screens; B, gateway between A and C. D and D′, electric passageways; N_2 and N_3, illuminated glass screens to be discriminated; E, openings to platforms at the sides of the experiment box; M and M′, cardboard shutters for closing these openings; U and R, electric keys for extinguishing lamps in H and P, respectively; K, stimulus key; I, inductorium.

an electric, 16 c.p., lamp (not shown in the figure) in a small box fitted with milk-glass windows. This lamp afforded light and warmth to the young chick during the intervals when it was in the hover box, and gave to a small area in the middle of this box approximately the temperature of the brooder in which the chicks were reared. The result of this was that chicks placed in O hovered near this lamp and thus rarely made, at either end of the box, any sounds which might influence the chick in the experiment box in its choice of a passageway back to O.

The inclined planes, G, of box O were replaced, early in the experiment, by two small platforms at the level of the floor of the experiment box. From these platforms the chicks hopped down directly to the floor of box O. This change was made because it was found that while chicks very readily walk up an inclined plane it is very difficult and apparently unnatural for them to walk down such planes. This difficulty becomes evident if one tries to imagine a man descending a steep incline with his body leaning far forward. The inclined planes, therefore, to the inconvenience of the experimenter, served rather to toll the chicks in box O upward toward the small doors of the experiment box than to give a means of descent for the chick which was escaping from the latter box. The platforms obviated this difficulty.

The illumination box, 107.8 x 40.2 x 23.2 cm., was divided lengthwise into two compartments by a light tight partition. The inside dimensions of each compartment were 107.8 x 19.3 x 23.2 cm. Each of these compartments held an incandescent lamp of the oval reflector type with frosted globe. These lamps were mounted on slides so that they could be moved easily along the millimeter scales, S. They were rated as of 50 c.p. When photometered at the close of the experiments the lamp in the right compartment had an intensity of 42.6 c.p., the one at the left 41.2 c.p. By moving the lamps along their millimeter scales they could be changed in position from 8.5 cm. to 103 cm. behind the opal glass screens, N_2 and N_3, so that a wide range of intensities of illumination was available.

As already stated, three different conditions of discrimination were used. For the condition termed "easy" one screen was illuminated by a lamp 33.5 cm. distant, the other screen was not illuminated. For "medium" discrimination one lamp was

at 23.5 cm., the other at 98.5 cm., and for difficult discrimination the lamps were placed respectively at 23.5 cm. and 53.5 cm. from the screens.

The experiment box was, as shown in the figure, somewhat narrower than the illumination box. It was divided into two compartments, A, 30 x 16.7 x 21.3 cm., and C, 46 (from partition B to the glass screens N_2 and N_3), x 30 x 21.3 cm. A damp pad of felt was placed on the floor of compartment A during the experiments and a similar pad in compartment C extended from the partition, B, to within 2 cm. of the electric wires. These pads were used to moisten the feet of the chicks, for when dry the horny epidermis served to protect the animals perfectly from the electric stimulus. The opening, shown in the partition, B, between the two compartments was closed by a mesh wire door which could be opened by lifting it vertically. That half of the floor of compartment C which was nearest the screens was wound with seventeen turns of phosphor bronze wire of No. 20 A.S. gauge. The distance between the successive wires was 1 cm. This wire was in circuit with the secondary coil of the inductorium, I, and the circuit could be closed by means of the electric key, K. A V-shaped partition divided the wired portion of this compartment into two passageways, D and D′. From these passageways two openings (of which one, marked E, is shown) gave means of egress for the chick to the platforms (see p. 113) and thence to the hover box, O. They were closed by the cardboard shutters M and M′.

The two opal flashed glass screens, N_2 and N_3, (N_1 was not used in the experiments) were each 12 cm. square. As already stated, the lamps were placed at different distances behind the two screens so that the latter differed from each other in brightness. Their relative brightnesses when photometered were roughly as follows:

For "easy" discrimination................	0[4]: 8.9 .
For medium discrimination................	1 :13.7 .
For difficult discrimination................	1 : 5.1 .

While one screen was not illuminated under the condition of easy discrimination it had a surface of rather high reflecting power and, since the experiments were made in diffused day-

[4] This screen was not illuminated. The zero is meant to indicate nothing more than that fact.

light, its value as perceived by the human eye was not darkness. This factor of reflected light was present throughout the experiments and made the difference in brightness of the two screens, as judged by the experimenter, much less than that indicated by the above ratio. Subjectively estimated the brightnesses of the two screens would stand, respectively, in the ratios 1 : 20 for easy discrimination, 1 : 4 for medium, and 1 : 2 for difficult.

A current of 2.1 amperes was supplied to the primary coil of the inductorium. The interruptions were 44 ± 5 per second. The positions of the secondary coil and the corresponding number of units of stimulation appear in table 1.[5]

TABLE I

Position of secondary	Units of stimulation
6	220
5	350
4	480
3	590

Method of the experiments. As a result of the experiments with the first group of chicks, Nos. 1–6 inclusive, it was found necessary to give all subsequent groups twenty trials in the experiment box in order that they might learn both ways of escape from it. The chick was first placed in compartment A of this box. The door in the partition was opened and it passed into compartment C. By drawing back the cardboard shutter M′ the small door, E, was opened through which the chick escaped to the hover box. In the next trial it escaped at the right and so on until the preliminary series had been completed.

There was no difference of brightness between the two screens during the preliminary tests. During the first five of such tests under the condition of easy discrimination there was no light behind either screen, during the second five trials both lamps were at 33.5 cm. and so on. During the first five tests of medium discrimination both lamps were at 98.5 cm., during the second five at 23.5 cm., and the distances 53.5 cm. and 23.5 cm. were similarly used in the preliminary tests of the difficult discrimination.

[5] For the calibration of the inductorium used in these experiments see the paper by Yerkes and Dodson, p. 467.

The experiments with the first group indicated also that chicks without preliminary training showed a very marked tendency to choose the more brightly illuminated screen. I therefore trained the chicks to escape to the hover box by choosing the *darker* screen. This was done also in order to make the results of my experiments more nearly comparable with those of Yerkes and Dodson, who trained their mice to select the

TABLE II

Positions of Darker Screen for Two Preference Series and Twenty-five Training Series

Subject................ Date................ Experiment................

Series	Tests												Remarks
	1	2	3	4	5	6	7	8	9	10	R	W	
A	l	r	l	r	l	r	l	r	l	r			
B	r	l	r	l	r	l	r	l	r	l			
1	r	l	r	l	r	l	r	l	r	l			
2	l	l	r	r	l	r	l	l	r	r			
3	r	r	l	r	l	l	r	l	r	l			
4	l	l	l	r	r	r	l	r	r	l			
5	r	l	r	l	r	l	r	l	r	l			
6	l	l	r	l	r	r	l	r	l	r			
7	r	l	l	l	r	r	r	l	r	l			
8	r	r	l	l	r	l	r	l	r	l			
9	r	r	r	l	l	l	r	l	r	l			
10	l	l	l	l	r	r	r	r	l	r			
11	r	l	r	r	r	l	l	l	r	l			
12	r	l	r	l	r	r	l	l	r	l			
13	r	l	r	l	l	l	r	r	r	l			
14	l	l	l	l	r	r	r	r	l	r			
15	r	l	r	r	r	l	l	l	r	l			
16	l	r	l	l	l	r	r	r	l	r			
17	r	r	r	r	l	l	l	l	r	l			
18	l	r	l	r	r	l	l	r	l	r			
19	r	l	r	l	r	l	r	l	r	l			
20	l	l	l	r	l	r	l	r	r	r			
21	r	l	l	r	r	l	l	r	r	l			
22	l	l	r	r	l	l	r	r	l	r			
23	r	l	l	l	l	r	r	r	r	l			
24	l	r	l	l	l	r	r	r	l	r			
25	r	r	r	r	l	l	l	l	r	l			

white box, since, in the preference tests, the dancers selected the black one in more than one-half the trials.[6]

As described above (p. 112), the preliminary series were followed by two series of ten trials each, called "preference series," and designated in Table II by the letters A and B. On the day following the completion of the "preference series," the training series was begun and they were continued until the chick had made twenty consecutive choices of the darker screen. The order of change of illumination of the two screens appears in table II. The letter l indicates that the screen at the left was the darker one, the letter r, that the one at the right was the darker.

Since the preference series were preceded by the twenty preliminary trials, in which the chick escaped from the experiment box by going alternately through the right and left passageways, the preference, so-called, was interfered with by the partially formed habit. Untrained chicks chose the brighter screen uniformly.

During the training series, if a chick chose the lighter passageway, it received an electric shock, whereupon it usually retreated from the wires, the door of the darker passageway was opened and through that it escaped to the hover box. Under this stimulus the chicks quickly learned to choose the darker screen under conditions of easy and medium discrimination. A few chicks were unable, even after many trials, to learn to choose the darker screen under the difficult condition of discrimination.

Results of the Experiments. The results of the experiments appear in table III. This table gives the three conditions of discrimination, easy, medium, and difficult, the relative strengths of the stimuli, the numbers by which the individual chicks were designated, and, opposite each of these, the number of trials *which preceded twenty consecutive correct choices*, or the number of trials "up to the point at which errors ceased."

In order to spare the reader an annoying repetition of the phrases, "easy, medium, and difficult conditions of discrimination," I shall sometimes refer to them, respectively, as great, medium, and slight differences of illumination or brightness of the two glass screens.

It is evident from table III that under the condition of easy discrimination the rate of learning is more rapid the stronger

[6] Yerkes and Dodson, loc. cit., p. 462.

TABLE III

GENERAL RESULTS OF EXPERIMENTS

Units of stimulation	Condition of discrimination		
	Easy Lamps at 33.5 cm. and darkness	Medium Lamps at 23.5 cm. and 98.5 cm	Difficult Lamps at 23.5 cm and 53.5 cm.
220 Secondary at 6		No. 61s– 90 trials " 62s– 90 " " 65i–150 " " 66s– 90 " Av. 105	
350 Secondary at 5	No. 7–50 trials " 8–30 " " 9–20 " " 11–60 " " 12–60 " Av. 44	No. 25–50 trials " 26–80 " " 27–80 " " 28–40 " " 29–50 " Av. 60	No. 43–230 trials " 44–180 " " 45–110 " " 46–230 " " 47–180 " " 48–100 " Av. 171.6
480 Secondary at 4	No. 13–20 trials " 14–20 " " 15–30 " " 16–20 " " 17–20 " " 18–20 " Av. 21.66	No. 31–30 trials " 32–50 " " 33–30 " " 34–50 " " 35–50 " " 36–30 " Av. 40	No. 37–120 trials " 38–140 " " 39– 90 " " 40–220 " " 41– 80 " " 42–Failed. Av. 130
590 Secondary at 3	No. 19–20 trials " 20–10 " " 21–10 " " 22–20 " " 23–10 " " 24–30 " Av. 16.66	No. 49–70 trials " 50–80 " " 51–40 " " 52–30 " " 53–50 " " 54–30 " Av. 50	No. 55–Died. " 56–Failed. " 57–70 trials " 58–50 " " 59–40 " " 60–Failed. Av. 53.33
590 Secondary at 3		No. 67s–40 trials " 68s–50 " } Av. 40 " 69s–30 " " 70i–80 " " 71i–20 " } Av. 53.33 " 72i–60 " Av. 46.66	

the stimulus. With a stimulus of 350 units an average of 44 trials was required before errors ceased, with 480 units 21.66

trials, and with 590 units only 16.66 trials. The same relation holds true for medium discrimination and stimuli of 220, 350 and 480 units, but when a stimulus of 590 units was employed the number of trials required for learning to make the discrimination *increased from* 40 *to* 50. In order to make certain that this increase in the number of learning trials was due only to the strength of the stimulus I repeated the test with a second group of six chicks and the average was practically the same, namely, 46.66 trials. With medium difference of brightness of the two screens, therefore, the optimal stimulus lies nearer the threshold than under the easy condition of discrimination.

The responses of the chicks to the third, or difficult, condition of discrimination are less easy to interpret. With the weakest stimulus used for this condition, 350 units, none of the six chicks failed, with the medium stimulus one failed, and with the strong stimulus two out of five failed. Moreover, the utmost patience was required of the experimenter in order that all should not fail. Each trial also required much more time than in medium and easy discrimination. If, however, we consider only the chicks that learned to make the difficult discrimination the relation stated for easy discrimination appears once more, i. e., the stronger the stimulus the more rapid the learning. It seems clear, therefore, that, with difficult discrimination, the strong stimuli divided the chicks into two groups, (1) those which after a few trials ceased to try to escape and would no longer step on the electric wires, and (2) those which chose with greater and greater caution and, therefore, learned to choose correctly after a small number of trials, each of which consumed much time.

To what shall we ascribe this dual result under the third condition of discrimination? It seemed possible that the chicks were divided into the two groups according to their sensitiveness to the electric stimulus. That is, the more sensitive chicks might learn most rapidly under the influence of a weak stimulus, be slow to learn under the influence of a strong one, and fail completely when under the influence of both a strong stimulus and a difficult condition of discrimination.

In order to answer this question twelve chicks were selected of which number six had a threshold of stimulation of 90 units and the remaining six of 150 units (relative values). The former

are designated by the letter s (sensitive) placed after their numbers in table III, the latter by the letter i (insensitive). Tests were then begun with three sensitive chicks, Nos. 61, 62, and 66, and with three insensitive ones, Nos. 63, 64, and 65, under the medium condition of discrimination and with a weak stimulus. Unfortunately, Nos. 63 and 64 died before the tests were completed. No. 65, however, required 150 trials for perfect discrimination while each of the sensitive chicks required exactly 90 trials. The loss of the two insensitive chicks makes a definite conclusion impossible, yet all our work with weak stimuli agrees with the result of the records of these four chicks. It is probable, therefore, that the chicks which were most sensitive to the electric stimulus were the ones which learned most rapidly under the influence of weak stimuli.

Let us turn now to the results of strong stimulation. Should the sensitive chicks be those which failed under the difficult condition of discrimination and strong stimuli they should be slowest to learn with the same stimuli and medium difference of illumination of the two screens, since it was already proved that a strong stimulus increased the learning rate under this condition. Three sensitive chicks (Nos. 67, 68, and 69) and three insensitive ones (Nos. 70, 71, and 72) were, therefore trained under this condition. An examination of their records shows that the sensitive chicks required an average of 40 trials for learning to discriminate between the two screens, while the insensitive ones required 53.33 trials. Evidently, therefore, sensitiveness to the stimulus was not the condition which prevented rapid learning under a strong stimulus.

At the close of these experiments with sensitive and insensitive chicks there seemed to be no explanation for the divergent results under the third, or difficult condition of discrimination. The *behavior* of the chicks indicated, however, that the pain stimulus impressed the memory of those that failed so deeply and permanently that, after a few experiences of it, they avoided the electric wires completely and would no longer attempt to escape from the experiment box. This observation, based on the chicks' behavior, receives striking confirmation from the records. The records of the successful chicks in the group 37–41, inclusive, show that in their first fifty trials each chick received an average of 20.4 pain stimuli, while chick 42, which failed,

received in the first fifty trials 30 such stimuli. These additional stimuli seemed to inhibit completely the impulse to enter the electric passageways. In the case of chicks 56–60, inclusive, only the average number of pain stimuli received during the first forty trials can be considered as chick 56 would not attempt to escape after the fortieth trial. In the first forty trials chicks 57, 58, and 59, which succeeded, each received an average of 15.3 pain stimuli. Chicks 56 and 60 received an average of 20.5 such stimuli *and failed*, while chick 55, which went to the wrong passageway in nine of the first ten trials, flew from the door of escape with such violence that he was injured in alighting. *Those chicks failed, therefore, which made more wrong choices in their early trials and consequently received more pain stimuli than their successful companions.* The additional repetitions of the stimulus seem to have stamped in the impression of the pain and to have caused the failures rather than a native difference of brain plasticity as I had supposed on observing the marked difference of behavior between successful and unsuccessful chicks. Here, as elsewhere, repetition seems to be prepotent in determining memory, if these smooth brained and extremely stupid creatures may be said to have memory. The difference between arousing extremely slow and cautious discrimination and inhibiting all efforts to escape lies, I believe, in the added number of pain stimuli given in early trials to the chicks which failed.

Records were kept of the sex of all the chicks used in the experiments but they revealed no correlation between sex and rate of learning. In fact the slow and rapid learners were distributed rather evenly between the two sexes.

Under the conditions of the experiments, it seemed probable that the heavier chicks received stronger electric stimuli than the lighter ones and therefore learned the more rapidly. But the weights of the chicks of several groups were recorded every three days during the period of experimentation without revealing differences between the heavier and the lighter individuals either in behavior or rate of learning. Again, there was no correlation between weight and sensitiveness to the current in the chicks whose threshold of sensitiveness was determined before training them.

I have shown that, for easy discrimination, increase of the inten-

sity of the stimulus is followed by decrease in learning rate, while, for medium discrimination an optimal intensity of stimulus is found, increase beyond which is followed by slower learning. Thus far my results and those of Yerkes and Dodson in the case of the dancing mouse seem to agree. In the case of the mouse under the difficult condition of discrimination it was found that the optimal stimulus approached much nearer the threshold than with medium difference of illumination between the two boxes. My results with chicks are in conflict with this unless, as has been done, the cases of failure to learn to discriminate are considered. Then it is found that, with the difficult condition of discrimination and the weakest stimulus, *none*, with the next greater strength of stimulus, *one*, and with the strongest stimulus *two chicks failed.* With slight difference of brightness between the two screens the strength of stimulus under whose influence no chicks fail to learn to discriminate is nearer the threshold than the optimal stimulus for the medium condition of discrimination. Perhaps this is as close agreement of the results for mice and for chicks as we should expect to find in animals so unlike. The behavior of the chicks was, however, the reverse of that of the mice. Yerkes writes:[7] "The behavior of the dancers varied with the strength of the stimulus to which they were subjected. They chose no less quickly in the case of the strong stimulus than in the case of the weak, but they were less careful in the former case and chose with less deliberation and certainty." My chicks, on the other hand, chose quickly with weak stimuli, but only after long delay with strong stimuli. A chick would sometimes require ten or fifteen minutes to make a choice in the latter case. This difference might perhaps be accounted for by the fact that, with the mouse, a moveable cardboard partition was used by which the space in which the animal could move was gradually restricted. Thus a choice of one passageway or the other was finally necessary. This device could not be used satisfactorily with chicks.

The record of one chick, which appeared to be perfectly normal when I began experiments with it, but died before they were completed, deserves notice. Its training series on successive days were as follows:

---

[7] Yerkes and Dodson, loc. cit., p. 476.

Daily series of tests	Choices Right	Choices Wrong
1	5	5
2	8	2
3	9	1
4	9	1
5	8	2
6	8	2
7	9	1
8	7	3

On the ninth day the chick was weak and would not choose either passageway. When I dissected it a large intestinal cyst was found in which there was much food and a fluid secretion. Such a cyst could have formed in a few days. But the important point is that the only sign of ill health in this chick for four days was the *decrease in the number of right choices.* On the fifth day physical signs of weakness appeared.

In conclusion, it is evident that within the limits of the stimuli which I used, the number of trials required by the chick to learn to choose consecutively the darker of two unequally illuminated screens, when discrimination is easy, decreases with an increase of stimulus. Under medium difficulty of discrimination the above law holds true only for the lower intensities of the stimuli which were used, or, in other words, the optimal stimulus recedes toward the threshold from 590 to 480 units. The above law for the condition of easy discrimination holds true for that of difficult discrimination if we consider only the records of the chicks which succeed in learning to make the discrimination. If, however, we consider only the chicks which fail, the optimal stimulus recedes once more to a point nearer the threshold of stimulation than in the case of medium discrimination. In other words, with the difficult condition of discrimination, strong stimuli divide the chicks into two groups, those which succeed in learning to discriminate by reason of more right choices at the beginning of the training series and consequently fewer pain stimuli, and those which fail because of fewer right choices and more pain stimuli in the earlier trials. So far as I determined the sensitiveness of the chicks it may be said that on the average the more sensitive chicks learned more rapidly both for strong and for weak stimuli.

No. 13 549

Reprinted from THE JOURNAL of NEUROLOGY AND PSYCHOLOGY Vol 20, No. 6.

THE RÔLE OF VISION IN THE MENTAL LIFE OF THE MOUSE

KARL T. WAUGH

From the Harvard Psychological Laboratory

TEN FIGURES

CONTENTS

I. PROBLEMS, METHODS AND RESULTS

The purpose of the investigation which is described in this paper was to answer the general question—What does the mouse receive from the outer world through the sense of vision, and of what importance in its life are the visual data so received?

The experimental work was done in the Harvard Psychological Laboratory between January, 1905, and March, 1907. I am

indebted to Professor R. M. Yerkes, under whose immediate supervision the work was carried on, for the suggestion of the problem and for much helpful criticism and advice throughout the course of the work. I wish also to make acknowledgment to Professor Münsterberg and Professor Holt for their advice and suggestions. The anatomical portion of the work was done in the Museum of Comparative Zoölogy at Harvard under the direction of Professor G. H. Parker, to whom I am indebted for aid and advice.

The general method pursued was that of presenting to the mouse a choice between two conditions, one being agreeable to the animal and the other disagreeable. In the majority of cases these were food and a slight electric shock.

PROBLEM 1. DISCRIMINATION OF LIGHT INTENSITY

A. Under indirect illumination

In experiments (a) and (b) under this head, five animals were used.

MOUSE	COLOR	SEX	AGE	DURATION OF EXPERIMENT
No. 1	gray	male	3½ months	Mar. 18–Apr. 17, 1905
No. 2	black	female	3½ months	Mar. 23–Apr. 27, 1905
No. 4	black	male	2 months	Mar. 23–Apr. 27, 1905
D	white[1]	male	3 months	Jan. 21–31, 1906
O	brown	male	3 months	Jan. 17–26, 1906

[1] All the white mice are albinos.

APPARATUS: This consisted of a wooden box measuring 52 cm. x 40 cm. on the inside, and 18 cm. deep. At one end of the box was a small opening fitted with a sliding door, which, when lifted, permitted the mouse to enter from the nest box N (fig. 1). At the other end of the experiment box were arranged two round tin boxes X and Y, each measuring 4½ cm. in diameter, covered with papers which differed in brightness, in color, or in both. The boxes were fitted into small wooden mounts fastened to two boards (22½ cm. x 18½ cm.). On these boards wires were placed, as is shown in fig. 1. These wires were then connected with a Porter inductorium. By closing the key, K, the experimenter could give a mouse whose feet rested upon two adjacent wires a slight shock.

METHOD: Food was placed in one of the boxes and the electric current was switched into the wires on the board which supported

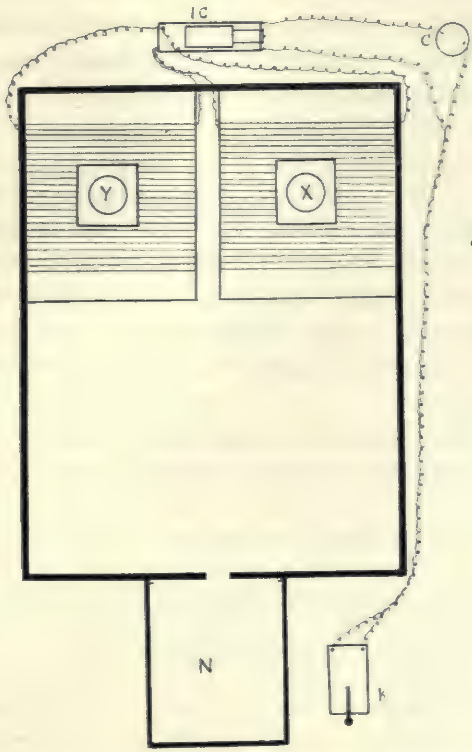


FIG. 1 Apparatus to test visual discrimination in the mouse. *N*, nest box; *X* and *Y*, food boxes; *I.C.* inductorium; *C*, dry cell; *K*, key.

the other box. In this way inducement was given to cause the animal to make use of what discriminative power it might possess for the purpose of avoiding the shock and obtaining the food. This, of course, presupposes the possibility of the formation of an association of shock with one intensity of light and of food with the other. The food used in this experiment was a small quantity of "force," which has little or no odor to enter as a disturbing factor.

An animal was placed in box *N* and the door was lifted. The

mouse would enter the experiment box and make a choice of X or Y, and after receiving a morsel of food or a shock, as the case might be, it would run or be driven back into box N. The boxes X and Y would then be interchanged and the current switched into the wires on the other side. When all was in readiness, a door between N and the main box was raised and the mouse was permitted to seek the food again.

It was considered a choice if the mouse touched the edge of either food-box. If he approached the wrong side first and received a shock and then ran over to the other side to get food, it was recorded as a wrong choice and the animal was forced to return to box N before making the next choice. Twenty trials were made each day with each mouse, and each choice was recorded as right or wrong according as the mouse obtained the food or the shock on first running out.

RESULTS: The following tables give the number of trials and the number of right and wrong choices:

Experiment (a) Black and white.

Black and white papers were pasted on the food-boxes.

MOUSE	FOOD IN	NO. TRIALS	RIGHT CHOICES	WRONG CHOICES	
D................	white	100	73	27	(see curve, fig. 2)
O................	white	100	83	17	(see curve, fig. 2)

Experiment (b) Light and dark varieties of a color.

Light and dark violet papers were substituted for the black and white.

MOUSE	FOOD IN	NO. TRIALS	RIGHT CHOICES	WRONG CHOICES	
No. 1........	Light violet	370	252	118	(see curve, fig. 2)
No. 2........	Light violet	80	42	38	
No. 4........	Light violet	230	152	78	(see curve, fig. 2)

CHECK EXPERIMENT: In order to make sure that the animals were using the visual sense in discriminating one paper from

the other, and not the sense of smell, check tests were made. With a mouse that seemed to give good evidence of discrimination, the food was placed in the darker box after having been in the lighter for earlier choices. In the case of mouse no. 1, after a training of 370 trials favoring the lighter violet, and after the boxes had been washed and clean papers pasted on them, the resulting choices were, in ten trials:

Light violet (no food)............8 Dark violet (food)............2

In fig. 2 are given the error curves for the discrimination of intensity. The ordinates represent the number of wrong choices in each twenty trials given on a particular day. The succession of days is marked on the axis of abscissas. D and O illustrate experiment (a); no. 1 and no. 4 illustrate experiment (b).

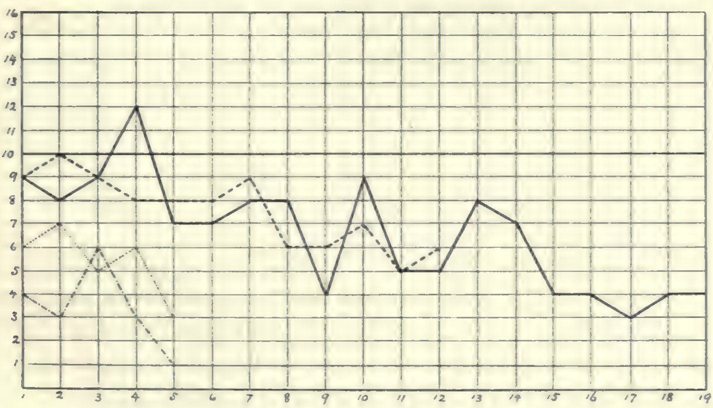


FIG. 2 Error curves for discrimination of light intensity. Ordinates represent number of wrong choices in twenty trials; abscissas represent days
——————————, Curve for mouse No. 1.
– – – – – – – – – – – – – , Curve for mouse No. 4.
- - - - - - - - - - - - - - - -, Curve for mouse D.
—·—·—·—·—·—·—·—, Curve for mouse O.

Experiment (c) Influence of background.

METHOD: In this experiment two animals, O and D, were used. They were trained for white by one hundred trials, in which the back and sides of the experiment box were covered with cards

(18 x 18 cm.). These were interchanged when the food boxes were changed so that the black food box always had a black background and the white box appeared against a white background. The choices during this training were not recorded.

Upon the completion of the training, changes were made in order to obtain an answer to the questions: does the mouse choose by discriminating between white and black boxes, or is it influenced also by the illumination of the whole field? Is food associated with object or with background? For this purpose white cards were placed behind the black box and black cards behind the white. Other conditions were later introduced for testing the nature of the association formed.

RESULTS:

White box against black background and vice versa

MOUSE	NO. TRIALS	RIGHT	WRONG	REMARKS
O	20	15	5	The last ten, 100 per cent
D	20	13	7	right, *i.e.*, white.

Uncovered tin boxes used. Backgrounds only changed

MOUSE	NO. TRIALS	RIGHT	WRONG	REMARKS
O	10	7	3	Great hesitation
D	10	10	0	

Mouse D was now tried without backgrounds but with the black and white papers on the food boxes. The result of twenty such choices was:

White box....................14 Black box........................6

The next experiment was with plain tin boxes without backgrounds, but with strips of paper (4 cm. x 18 cm.) laid crosswise on the floor of the experiment box directly in front of the boards which carried the electric wires. Black paper was placed on the one side and white on the other.

Under these conditions mouse O in ten trials crossed over the white paper to reach the tin box nine times, the black paper, once.

In the next experiment, light gray paper was substituted for the white with the following results.

MOUSE	NO. TRIALS	LIGHT GRAY	BLACK
O	26	12	14
D	10	6	4

The strips were now taken up and the tin boxes were covered respectively with light gray and dark gray papers. No backgrounds were used.

The experiment resulted as follows: By mouse D, in 40 trials, the light gray was chosen 19 times, the dark gray 21 times.

Experiment (*d*). *Selection of yarns.*

Preference of mice for light or dark yarns obviously depends upon their power of discrimination.

METHOD: Neither food nor electric shock was used in this experiment. Advantage was taken of the opportunity afforded by the instinct of a mother mouse to make a warm nest for her litter. A black mouse, X, about six months old, with five little ones, was taken as subject. When the young mice were a little over a week old, pieces of yarn were hung in the cage and some of the cotton was removed from the nest. The order in which the mouse took the different yarns to replenish the nest was recorded.

The yarns used were white, black, and two shades of gray. These were hung in the cage in a row, 4 cm. apart and at a distance of 15 cm. from the entrance to the nest. The lower ends rested on the floor of the cage. After a set had been pulled down by the mouse and taken into the nest, four other pieces were hung up in a different order. After three or four sets had thus been taken they were removed from the nest and the experiment was repeated.

In an entire series all the possible arrangements of the four shades were made. There being six possible permutations of the four yarns and four places, there were 96 selections in the series, each yarn appearing in each place six times. The order in choosing the four positions was recorded also, in order to show what influence the habit of first seeking a certain locality might have.

RESULTS: The table gives the number of times each shade was selected first, second, third, and fourth or last.

YARN	FIRST CHOICE	SECOND CHOICE	THIRD CHOICE	FOURTH CHOICE
Black	8	7	7	2
Dark gray	6	6	7	5
Light gray	4	2	6	12
White	6	9	4	5

As seen from the table, the order of preference is (1) black, (2) white, (3) dark gray, (4) light gray.

The nest was next turned through 180 degrees and the yarns were hung in the back of the cage, opposite where they had been before, and another complete series of records of choices was obtained in the same manner. The results of this second series are summarized in the following table:

YARN	FIRST CHOICE	SECOND CHOICE	THIRD CHOICE	FOURTH CHOICE
Black	7	6	8	3
Dark gray	5	8	7	4
Light gray	3	4	3	14
White	9	6	6	3

The order of preference as shown from this table is (1) white, (2) black, (3) dark gray, (4) light gray.

The leaving of light gray till last in both series was so interesting that it was thought well to make a check test to learn whether the taste or odor of that particular dye was determining the animal's choice rather than the shade. A gray yarn was made by twisting together a strand from the black and one from the white, the two preferred yarns, and a set of 24 choices of the three yarns, black, white and gray, was obtained.

The results were as follows:

YARN	FIRST CHOICE	SECOND CHOICE	LAST
White	5	0	1
Black	1	3	2
Gray (black and white)	0	3	3

Conclusions: (From the experiments under problem, 1, A.) That the mouse discriminates between light and dark objects under indirect illumination is evident from experiments (a) and (b).

Experiment (c) shows that both object and background are influential in determining the reaction.

The albino mouse was influenced by the environment more than the brown mouse. This is shown in the case of the white mouse D making 100 per cent right choices, *i.e.*, choices of white background when the food boxes were uncovered. This result is quite in harmony with the biological theory of protective coloration.

B. Under direct illumination

Apparatus: In fig. 3 is shown a view of the apparatus used in these experiments. It consists of two parts, an experiment box (32 cm. x 52 cm.) and a light box (32 cm. x 98 cm.) Between these two parts is a slide carrying ray filters. *A* is a nest box (29½ cm. x 18 cm. inside) from which the animal can enter the compartment *B* through a door *I*. From *B* (20 cm. x 17 cm.) it cannot pass back into *A* directly, but must enter one of the smaller compartments in front, which open into alleys on each side. From one of these alleys the animal reaches the nest box by a gate *O*. The two small compartments, (each 14½ cm. x 8 cm.) which may be entered from *B*, are illuminated by the light from electric lamps in the light box, which enters the compartments at *G* and *R* through two apertures each 6½ cm. square. These apertures, in the experiments now to be described, were covered with ground glass. In the light box the lamps can be moved back and forth to give the required differences in intensity, their distance from the ground glass being measured on a scale *S*. The light box is divided lengthwise by a partition which insures the illumination of each aperture by the appropriate lamp only. The slide carries three rectangular cells (15x16 x 6 cm.) separated from one another by pieces of felt. These, filled with colored media, can be used as ray filters for tests of color discrimination. During the present experiments they remained empty.

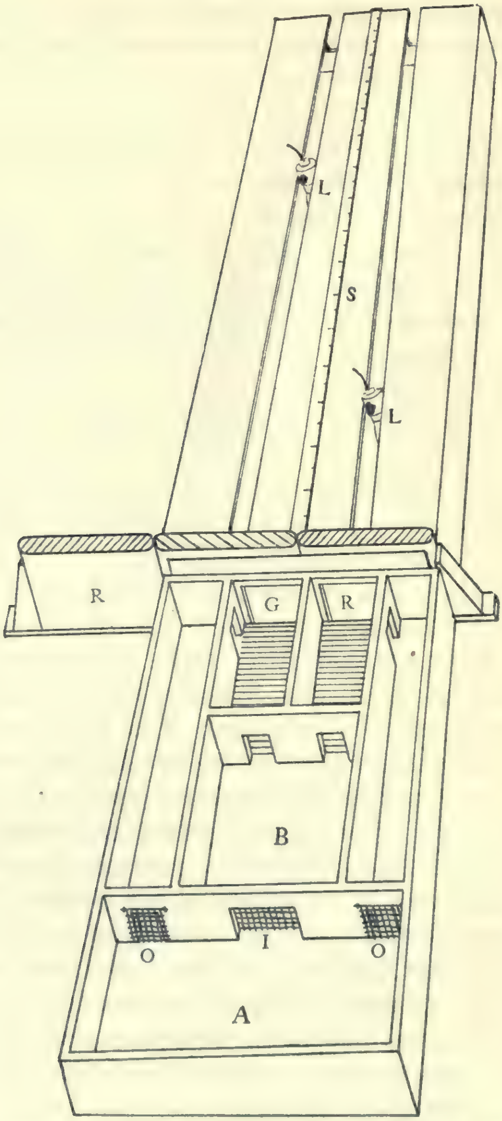


FIG. 3 Visual discrimination apparatus. *A*, nest box; *B*, entrance chamber; *R*, *R*, red ray-filter; *G*, green ray-filter; *L*, *L*, incandescent lamps in light box; *S*, millimeter scale on light box; *I*, door between *A* and *B*; *O*, *O*, doors between alleys and *A*. (Yerkes, The Dancing Mouse, p. 153. 1907. The Macmillan Co., N. Y.)

METHOD: The method is very similar to that previously described. Two animals were used:

MOUSE	COLOR	SEX	AGE
U	black	male	About 4 months
V	white	male	5 months

These animals were trained to enter the brighter of the two compartments. Food was kept in both alleys near *O*, but the exit into the alley on the side of the weaker light was closed by a piece of glass which could be slipped into the partition just above the exit and pushed down. A shock was given whenever the mouse entered the darker compartment. From the brighter compartment the animal was allowed to pass unmolested into the alley, obtain food and enter the nest box through the passage *O*. After receiving a shock the animal was apt to run quickly out of the darker compartment and into the other one.

The wire door at *I* permitted passage only into *B*, and those at *O*, *O*, only into *A*. The mice soon learned that the food was to be obtained only by passing from *B* through one of the small compartments into the alley.

Twenty experiments were made with a mouse each day. Sometimes it was found necessary to urge the animal to make a choice by gradually narrowing the space in compartment *B* with a thin board, placed vertically across the compartment.

The experiment was begun with one light at a distance of 34 cm. from the ground glass, and the other at 54 cm. After each choice, or sometimes two or three choices, the light that had been at 34 cm. was moved to 54 cm. and the other moved up to 34 cm. thus making the other compartment the brighter. At the same time the piece of glass which had blocked the exit was removed and pushed into the other slip, blocking the exit on the opposite side.

RESULTS: ("Right" indicates choice of the brighter of the two lights).

Lights at 34 cm. and 54 cm.

MOUSE		CHOICES 1ST DAY	2ND	3RD	4TH	5TH	TOTAL
U	Right	15	17	14	17	18	81
	Wrong	5	3	3	3	2	19
V	Right	14	14	13	17	18	74
	Wrong	6	6	7	3	4	26

Lights at 34 cm. and 40 cm.

MOUSE		CHOICES 1ST DAY	2ND	3RD	4TH	5TH	TOTAL
U	Right	15	12	13	13	14	57
	Wrong	5	8	7	7	6	33
V	Right	11	13	13	12	13	62
	Wrong	9	7	7	8	7	38

CONCLUSIONS: The mouse discriminates between differences in the brightness of white light; the less the objective difference, the greater the difficulty in discrimination.

The discrimination of the albino mouse is slightly inferior to that of the mouse with black eyes.

PROBLEM 2. COLOR DISCRIMINATION

A. Under indirect illumination

Experiment (a). Discrimination of colored objects.

APPARATUS AND METHOD: The apparatus used in this experiment was the same as that already described under problem 1, A, (a) and (b) (see p. 550). The colors chosen were an orange-red and a blue (Bradley papers). These were judged to be of equal intensity by several members of the laboratory. The papers were pasted on tin boxes and the experiment was conducted as in the intensity-discrimination experiment. The following eleven mice were used:

MOUSE	COLOR[1]	SEX	AGE[2]	DATE OF EXPERIMENT
No. 1	gray	male	2 months	Jan. 27–Feb. 20, 1905
No. 3	gray	male	7 weeks	Feb. 17–25, 1905
W F	white	female	7 weeks	Feb. 2–25, 1905
No. 5	gray	male	9 weeks	Nov. 20–Dec. 2, 1905
B M	brown	male	2 months	Oct. 10–Nov. 8, 1905
B S	brown	male	1 month	Oct. 10–Nov. 8, 1905
A	white	male	adult	Dec. 2-4, 1905
B	white	male	adult	Dec. 2-4, 1905
C	white	male	1 month	Nov. 9–Dec. 10, 1905
D	white	male	5 weeks	Nov. 9–Dec. 10, 1905
E	white	male	7 weeks	Nov. 29–Dec. 10, 1905

[1] All the white mice are albinos.

[2] The ages are approximate. An adult mouse is over 2 months old. Age given is age at the time experiment was begun.

Marked individual differences in the animals made it possible to get a much greater number of choices with some than with others. The ease with which a motor habit may be formed makes all the difference between a good subject and a poor one. Mouse no. 1 early formed the habit of running directly to one of the tin boxes, taking a piece of food (if he happened to select the box which contained food) and running back with the morsel to box *N*. He would be eating the last piece of food while I was changing the boxes and switching the current to the other board. Thus he saved me much time. He always made his full assignment of twenty choices per day. Some of the other mice would follow along the edge of the experiment box, pause a long time in the corners and wash their faces, or they would take the food in such a way as to give no satisfactory evidence of choice. Throughout these red-blue discrimination tests the food was kept in the red box. At frequent intervals the papers were removed and fresh ones pasted on, in order to obviate the influence of odor. The experiment box was so adjusted with reference to the windows of the room, that the two halves were equally illuminated.

Results: The following table gives a summary of the results of the red-blue discrimination tests:

MOUSE	NO. TRIALS	RIGHT (RED)	WRONG (BLUE)	PER CENT RIGHT
No. 1	480	320	160	67
No. 3	55	38	17	70
W F	185	104	81	56
No. 5	91	49	42	54
B M	210	160	50	76
B S	28	15	13	54
A.	12	9	3	75
B	10	6	4	60
C	55	29	26	53
D	44	22	22	50
E	120	68	52	57

The figures in this table represent the total number of right and of wrong choices during the training. The improvement in discrimination and the stage of success finally attained are shown in the curves of fig. 4. The results for one mouse are presented as typical. On the axes of ordinates are represented the

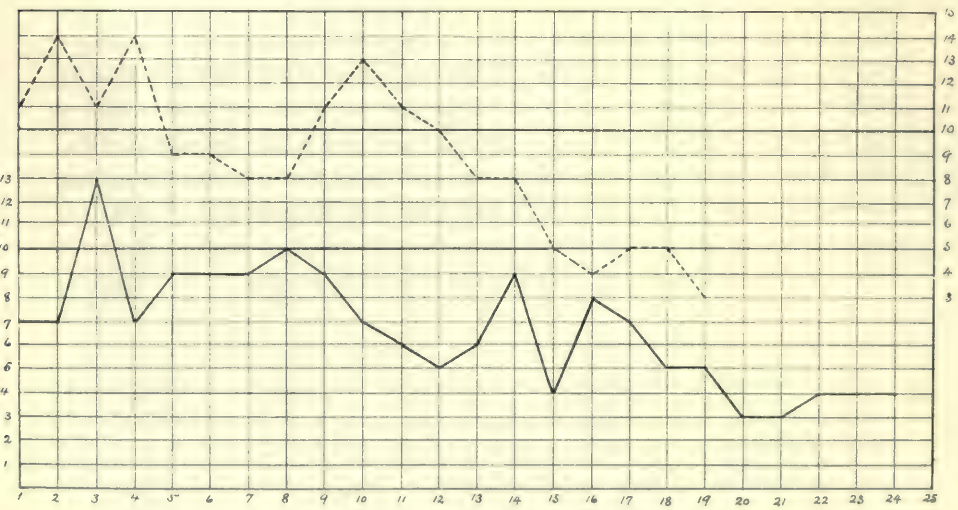


FIG. 4 Curves of learning. Number of errors made daily is represented on the axis of ordinates. Number of days is represented on the axis of abscissas. Lower curve is the error curve for one mouse in red-blue discrimination. Upper curve is the curve of dissociation and association for the same mouse.

number of errors made daily in twenty choices. Successive days on which sets of choices were made are represented on the axis of abscissas. The horizontal line at 10 may be regarded as the line of no discrimination.

Check tests: To make sure that vision was being used by the animals in discriminating, rather than smell, food was placed in the blue-covered box and the red box was emptied. With mouse no. 1 after the training for red of 400 trials, the result was, that, in the first hundred choices, the mouse selected the empty red box first 59 times in preference to the blue which contained the food.

Keeping the food in the blue, the experiment was continued in order to discover how rapidly an association of one color with food, formed through training, could be changed to an association involving a preference for the previously avoided color. The dotted curve of fig. 4 represents the result of 380 trials. It is an error curve in which choices of red are considered "wrong."

With another mouse, B M, a series of experiments was made, in which the check test consisted in changing the food into the blue box for from three to five choices, after each ten choices with the food in the red. This was done after 190 choices in training for red had already been made. The results are given in the following table. The first column gives the number of the test; the second gives the number of right choices out of ten with the food in red; the last two columns give the results with the food transferred to the blue.

NO.	FOOD IN RED	FOOD IN BLUE	
	Right Choices	No. trials	Red chosen
1	8	5	5
2	9	5	3
3	8	5	5
4	10	5	4
5	7	5	3
6	8	5	5
7	4	5	3
8	9	3	0
Total	63	38	28

Experiment (b) Selection of colored yarns.

METHOD: The apparatus as well as the method here used was the same as that used in the experiment on the selection of gray yarns (see p. 555). Three animals, all of them mothers of litters, were subjects in the experiment.

MOUSE	COLOR	SEX	AGE	DATE OF EXPERIMENT
K	white	female	3 months	Mch.–Apr., 1906
X	black	female	6 months	Jan., 1907
Y	white	female	cir. 1 year	Jan., 1907

Four colors, blue, red, yellow and green, of as full saturation as possible, were selected from among the yarns used in the laboratory for testing color blindness. These were hung in a row in the back part of the mouse's cage, all at the same distance from the opening into the nest. After the four pieces had been pulled down and carried into the nest one after another, another set was arranged in a different order, as in the previous experiment with gray yarns.

RESULTS: These are presented in tables, each table representing a series of 96 selections of yarns.

Mouse K

COLOR	NO. OF TIMES SELECTED				ORDER OF PREFERENCE
	1st	2nd	3rd	4th	
Blue	6	3	10	5	3
Red	10	5	3	6	1
Yellow	6	11	2	5	2
Green	2	5	9	8	4

Mouse X

COLOR	NO. OF TIMES SELECTED				ORDER OF PREFERENCE
	1st	2nd	3rd	4th	
Blue	7	5	4	8	3
Red	6	11	3	4	2
Yellow	11	3	7	3	1
Green	0	5	10	9	4

The order of preference is obtained by comparing the numbers which represent the *preference-value* of each color. Such a number may be obtained by adding together the number of times a color is chosen first $\times$ 4, the times it is chosen second $\times$ 3, the times chosen third $\times$ 2 and the number of times chosen last. Thus for K, the preference value of blue is: $6 \times 4 + 3 \times 3 + 10 \times 2 + 5 = 58$. In the same way the preference value of red is found to be 67, of yellow 66, and of green 49. For X the values are: blue 59, red 67, yellow 70, and green 44.

This preference-value is useful in showing the constancy throughout the series. Thus the whole set of results for X was divided into halves and the preference values for each half were as follows:

	FIRST HALF	SECOND HALF	WHOLE
Blue	26	33	59
Red	34	33	67
Yellow	35	35	70
Green	25	19	44

CONCLUSIONS: The mouse can learn to associate food or electric shock with red or blue objects. The connections thus formed may be disassociated and an association formed with another color.

In albino mice, color discrimination is poor.

Red and yellow are preferred to blue and green, and of the latter two, blue is preferred to green.

Whether the discrimination involved is true color discrimination, after the fashion of that in human beings, can not be discovered, but we may call it color discrimination so long as it answers the practical purposes of the mouse in distinguishing between such objects as it is likely to meet with in its habitat.

If it be claimed that colors of the red end of the spectrum are preferred by the black mice because they seem to them the darker, we would suggest a correlation with the results obtained in the matter of the preference for gray yarns (see pp. 556). The same mouse, X, appeared in the two experiments. White and black were both preferred to either of the intermediate grays.

B. Under direct illumination

Apparatus: The same as that in problem 1, B, fig. 3. The colors used were red, blue and green, obtained by means of the following ray filters and glasses:

For green—a saturated solution of nickel nitrate.

For blue—solution of copper ammonia sulphate.

For red—pieces of ruby glass placed in the cell.

The three cells in the slide between the light box and the experiment box were filled with the solutions, the two on the outside containing the same solution. By moving the slide a little to one side, the relative positions of the colors were changed, the one previously on the right now appearing on the left. The cell containing the ruby glass was filled with water in order that by the effect of the refraction it might appear to be the same distance away as the other colors.

In all, fourteen animals were made use of in this experiment:

MOUSE	COLOR	SEX	AGE	DATE OF EXPERIMENT
Q	white	male	adult	Apr. 23–27, 1906
J	white	male	adult	Apr. 17–May 17, 1906
N	black	male	adult	Apr. 28–May 7, 1906
R	white	male	adult	Apr. 12–May 25, 1906
G	brown	female	adult	Apr. 5–May 19, 1906
C	white	male	5 months	Mar. 26–May 24, 1906
M	brown	male	? (wild)	Apr. 6–9, 1906
D	white	male	7 months	Mar. 26–May 24, 1906
F	brown	male	adult	Apr. 9–May 19, 1906
T	gray	male	4 months	Nov. 9–Dec. 17, 1906
U	black	male	4 months	Nov. 25–Jan. 14, 1907
Xa	gray and white[1]	male	1 month	Jan. 9–21, 1907
V	white	male	6 months	Nov. 16–Dec. 10, 1906
Z	white	male	6 months	Nov. 14–Dec. 10, 1906

[1] The gray and white mouse had pigmented eyes.

Method: The first two colors used were green and red. One of the electric lamps was moved until the intensities of the two colors appeared equal. Judgment was given on this point by five members of the laboratory, after each had been in the dark-room for five minutes.

A preliminary test of 50 trials was made with each animal to see if any natural preference for either of the two colored lights existed. The mice were found negative in this respect. Under the circumstances, it is evident that the problem resolves itself into the question whether the mouse can be trained to prefer one quality of light to another. This is to be done by the association of pleasure or pain with whatever distinguishing characteristics the lights may present to the eye of the animal. Just what the distinguishing characteristics are—what factors the mouse uses in discriminating—may be suggested by the use of check tests, in which the relative brightness of the lights is varied. In this respect the apparatus of this experiment is more satisfactory than that of the first series under this problem. Yet it leaves something to be desired, for if the animal's choices are determined wholly by intensity, this fact would become apparent, but if they are determined by more than one factor (*e.g.*, quality and intensity) as seems probable, then we can hope for results only on the supposition that a point of non-discrimination may be found, or better, a point of least discrimination, where a certain intensity tends to counteract the quality which would act in part as a determining factor.

RESULTS:

Red-green discrimination

MOUSE	TRAINED FOR	NO. CHOICES	RIGHT	WRONG
Q	green	100	57	43
J	red	100	60	40
N	red	100	58	42

Green-blue discrimination

MOUSE	TRAINED FOR	NO. CHOICES	RIGHT	WRONG
R	green	100	42	58
G	green	100	50	50
C	green	100	49	51
M	green	60	29	31
D	green	100	50	50
F	green	80	41	39

The next problem undertaken was that of ascertaining whether the mouse discriminates between white and red lights when they are of about the same degree of brightness.

For this experiment four incandescent lamps were used as sources of light. Two, measuring 4 c.p. each, were used back of ruby glass to yield the red light, and two measuring 13 c.p. each back of ground glass to yield the white light stimulus. The positions of these lamps in the light box were changed as indicated in the following tables of results.

Lamp of 13 c.p. 34 cm. from red glass; lamp of 4 c.p. 80 cm. from white glass (This yielded a red and a white stimulus which seemed of equal intensity to the human observer)

MOUSE	COLOR	NO. OF TIMES CHOSEN										TOTAL
T........	White	6	4	8	6	7	7	6	8	6	7	65
T........	Red	4	6	2	4	3	3	4	2	4	3	35

Lamp of 13 c.p. 14 cm. from red glass; lamp of 4 c.p. 90 cm. from white glass

MOUSE	COLOR	NO. OF TIMES CHOSEN												TOTAL
U........	White	8	9	8	8	7	7	8	8	10	8	9	10	100
U........	Red	2	1	2	2	3	3	2	2	0	2	1	0	20
T........	White	8	6	7	7	–	–	–	–	–	–	–	–	28
T........	Red	2	4	3	3	–	–	–	–	–	–	–	–	12

Lamp of 13 c.p. 54 cm. from the red glass; lamp of 4 c.p. 70 cm. from white glass

MOUSE	COLOR	CHOICES		TOTAL	MOUSE	COLOR	HOICES		TOTAL
U........	White	7	10	17	T..........	White	8	9	17
U........	Red	3	0	3	T..........	Red	2	1	3

Lamp of 13 c.p. 14 cm. from red glass (one of the two thicknesses of ruby glass used previously removed); lamp of 4 c.p. 90 cm. from white glass

MOUSE	COLOR	CHOICES		TOTAL
U.....................	White	8	8	16
U.....................	Red	2	2	4

Lamp of 13 c.p. 14 cm. from red glass (single thickness); no light through white glass

MOUSE	COLOR	CHOICES			TOTAL
U	White	6	3	6	15
U	Red	4	7	4	15

Lamp of 16 c.p. 14 cm. from red glass; lamp of 1.69 c.p. 90 cm. from white glass.

MOUSE	COLOR	CHOICES		TOTAL
U	White	8	9	17
U	Red	2	1	3

Lamp of 16 c.p. 14 cm. from red glass; Lamp of 1.69 c.p. 90 cm. from white glass. (With glasses at exits, so placed as not to reflect light)

MOUSE	COLOR	CHOICES										TOTAL
U	White	4	9	6	5	6	6	6	7	–	–	49
U	Red	6	1	4	5	4	4	4	3	–	–	31
Xa	White	7	6	5	8	5	5	5	4	5	6	56
Xa	Red	3	4	5	2	5	5	5	6	5	4	44

Lamp of 16 c.p. 34 cm. from red glass; lamp of 1.69 c.p. 60 c.m. from white glass

MOUSE	COLOR	CHOICES			TOTAL
Xa	White	5	4	6	15
Xa	Red	5	6	4	15

Lamp of 13 c.p. 34 cm. from red glass; lamp of 4 c.p. 80 cm. from white glass (Brightnesses equal for human eye)

MOUSE	COLOR	CHOICES										TOTAL
V	White	5	5	6	6	6	5	8	4	5	7	57
V	Red	5	5	4	4	4	5	2	6	5	3	43
Z	White	6	8	4	4	7	3	5	5	5	6	53
Z	Red	4	2	6	6	3	7	5	5	5	4	47

CONCLUSIONS: Our results do not indicate any ability to discriminate between red and green or between blue and green. The black-eyed mice discriminate between red and white light when they are of equal brightness to the human eye. When the red is made darker the discrimination is slightly improved. When the red is made brighter the discrimination is not so good. The power to discriminate seems to fall away as the brightness of the red relative to the white increases for the human observer.

PROBLEM 3. PERCEPTION OF FORM

APPARATUS AND METHOD: In these experiments the apparatus used was the same as that already described in connection with the problem of the discrimination of direct light stimuli. (see p. 557)

Pieces of black cards were substituted for the liquids in the cells, and in these cards were cut holes of the forms desired for the test. The forms selected were, a circle, 4 cm. in diameter, and an X-shaped figure which was inscribable in a square of 4 cm. diameter (fig. 5). I made these two figures of equal area in

FIG. 5 Cards for study of perception of form.

order that difference in the amount of light passing through them might not serve as a condition of discrimination.

Two incandescent lamps were used as sources of light. Each of these was of 13 candle-power, as tested by the Lummer-Brodhun photometer. The lamps were placed in the light box at a distance of 50 cm. from the apertures. Additional pieces of ground glass were placed against the black cards inside the cells, in order

to make the illumination of the apertures more homogeneous, as well as to reduce the light.

The electric shock (as punishment for a wrong choice) was given in the compartment with the X-aperture, the side exit on that side being closed with a piece of glass. When the animal entered the compartment marked by the circle, it was allowed to pass into the alley and to obtain food.

The circle and X were interchanged at intervals, usually after three or four choices had been made. This was accomplished by sliding the carriage a few inches to the right or left so that another card with an X-aperture would come into the field, taking the circle's place, the circle having moved into the position of the former X. Ten choices were obtained each day. Four mice were used in this experiment.

MOUSE	COLOR	SEX	AGE
Ya	white	female	3 months
Yi	white	female	3 months
X	black	female	5 months
Xd	black	female	6 weeks

RESULTS: In the following table, R stands for right choices, *i.e.*, circle; W for wrong, *i.e.*, X-aperture.

DAY	Ya		Yi		X		Xd	
	R	W	R	W	R	W	R	W
1st	4	6	4	6	3	7	8	2
2nd	7	3	7	3	6	4	4	6
3rd	8	2	4	6	6	4	8	2
4th	8	2	10	0	6	4	7	3
5th	8	2	6	4	5	5	6	4
6th	6	4	6	4	–	–	6	4
7th	8	2	8	2	–	–	–	–
8th	7	3	6	4	–	–	–	–
9th	7	3	7	3	–	–	–	–
10th	6	4	–	–	–	–	–	–
11th	7	3	–	–	–	–	–	–
12th	7	3	–	–	–	–	–	–
13th	7	3	–	–	–	–	–	–
Totals	90	40	58	32	26	24	39	21

Conclusions: As may be seen from the table, perception of form does not seem to be very well developed in the mouse. Only in the case of Ya, may we feel justified in concluding that discrimination was present, and even here errors were not eliminated up to the 130th choice. That the mouse is able to form an association of object with food or shock is shown by the experiments in intensity discrimination. Evidently the mouse tends to depend upon the size of the illuminated area or the intensity of the light.

When a strange mouse is introduced into the cage of another mouse there seems to be no recognition of the nature of the intruder by vision. Not until the home mouse can touch and smell of the stranger, does there seem to be any knowledge of whether he is an enemy or the mate of the home mouse. One might suppose that differences in the form of the animal would be noticed at some little distance. It is by form that human beings known one another, different expressions of the face being in the last analysis minor differences in form.

PROBLEM 4. PERCEPTION OF DISTANCE OF OBJECT FROM ANIMAL

Apparatus: A wooden disk 10 cm. in diameter, supported in the center by a column $2\frac{1}{2}$ cm. in diameter, which passes through

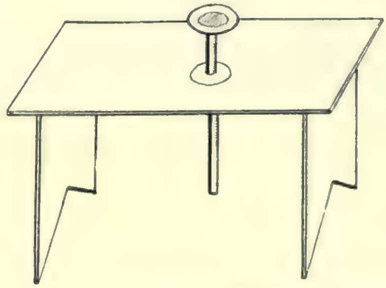


Fig. 6 Table for study of perception of distance.

a round hole in the top of a bench. The height of the disk above the bench can be adjusted by sliding the column up or down.

METHOD: A mouse was placed upon the disk, raised to a certain height above the bench and allowed to jump down. The time which elapsed from the instant the mouse was placed on the disk till he jumped off was recorded with a stop-watch. The time was taken as a measure of the animal's ability to perceive distance. The mice were active and restless and continually peered down over the edge of the board as though measuring the distance of the leap.

Nine animals were used in this experiment:

MOUSE	COLOR	SEX	AGE	DATES OF EXPERIMENT
E	white	male	7 weeks	Nov. 22–Dec. 14, 1905
BM	brown	male	9 weeks	Nov. 22–Dec. 14, 1905
D	white	male	7 weeks	Nov. 22–Dec. 14, 1905
U	black	male	cir. 3 months	Jan., 1907
Xb	gray	female	6 weeks	Jan., 1907
Wa	white	male	3 months	Jan., 1907
Ye	white	female	3½ months	Jan., 1907
Xd	black	male	6 weeks	Jan., 1907
T	gray	male	cir. 6 months	Jan., 1907

With each mouse five trials were made daily, at each of several different heights of the disk, and an average was taken of the time records for these five. Usually if an animal was not off the disk within two minutes it was recorded as unwilling to venture the leap.

RESULTS:

With plain bench-top below disk

HEIGHT OF DISK	18 CM.	15 CM.	12 CM.	10 CM.	8 CM.	6 CM.	4 CM.
	sec.	sec.	sec.	sec.	sec.	sec.	sec.
E's time	35	31	19	7.5	6.4	5.6	2
BM's time	—	45	7	4.5	3.5	3.4	—

Large red and blue sheets of paper, when placed over the top of the bench, gave similar results.

When sawdust and scraps of paper were scattered on the bench, the time was slightly lessened. This, however, may be due to the

greater ease of jumping off, which had been acquired through practice.

The disk with its supporting column was inverted and inserted in the bench-top from below. This was done because it was feared that the animal in peering over the edge of the disk might be influenced by seeing the column. A large plate of glass was fixed under the disk in its new position.

With disk inverted and glass below it

MOUSE	10 CM.	8 CM.	6 CM.	4 CM.
	sec.	*sec.*	*sec.*	*sec.*
E	41	18	—	—
D	120	120	120	7.6

At 4 cm. the vibrissae touched the glass. D's average, even at this height, was unusually long.

When the glass was lowered to a distance of 10 cm. above the floor the results were similar to those just presented.

With a board, instead of the glass, placed below the disk

MOUSE	6 CM.	5 CM.	4 CM.	3 CM.	2 CM.
	sec.	*sec.*	*sec.*	*sec.*	*sec.*
U	120	32	22	—	–
U (2d series)	—	75	31	17	–
Xd	120	120	45	36	2
Xb	120	120	120	5	
Xc	120	69	10	–	–
Xc (2d series)	—	60	33	–	–
T	120	59	32	–	–

The effect of a white and black bench surface upon the animal's reactions was next tested.

METHOD: With the disk in position above the bench, white and black papers were spread over the bench in such a way that one half of the disk was above white, the other half above black paper. A large piece of plate glass having a round hole in the center for

the column to pass through, was placed over the papers in order that the surfaces might not be soiled by the animals, as well as to keep the papers flat and in position. A circular disk of polished steel, 7 cm. in diameter, was fitted closely around the column over the glass. This was intended to prevent the animals from jumping down too close to the column. A small wooden disk wrapped with electric wires was fastened to the top of the disk to bring about a quicker reaction by making too long a stay on the disk slightly unpleasant for the mouse. The wires from this small disk passed through the larger disk and were sunk into the sides of the column, thence they passed downward through the bench top to the floor where they were connected with an induction coil and dry cell.

The time records are of little importance here, for the purpose of the experiment was merely to observe the choice of the black or white surface as a landing place. It was assumed that the animal would jump down on the surface which appeared to it the nearer.

Results:

Height of disk 4 cm. above bench

MOUSE	TRIAL	PLACE	TIME	STIMULUS
			sec.	
Wa	1	white	10	shock
Wa	2	white	3	no shock
Wa	3	white	5	no shock
Wa	4	joining	10	no shock
Wa	5	white	3	no shock
Wa	6	white	3	no shock

Disk turned 180 degrees

MOUSE	TRIAL	PLACE	TIME	STIMULUS
			sec.	
Wa	7	white	5	no shock
Wa	8	white	67	shock
Wa	9	white	320	shock

The next day another series was tried with the disk in its original position and the whole apparatus turned around 180 degrees.

Disk 5 cm. above the bench

MOUSE	TRIAL	PLACE	TIME	STIMULUS
			sec.	
Wa..........................	1	white	20	no shock
Wa..........................	2	white	5	no shock
Wa..........................	3	white	300	shock

Disk turned 180 degrees

MOUSE	TRIAL	PLACE	TIME	STIMULUS
			sec.	
Wa..........................	4	white	120	shock
Wa..........................	5	white	186	shock
Wa..........................	6	white	1200	shock
Ye..........................	1	white	6	no shock
Ye..........................	2	black	250	shock
Ye..........................	3	joining	15	shock
Ye..........................	4	black	18	shock
Ye..........................	5	joining	17	shock

Table turned 180 degrees

MOUSE	TRIAL	PLACE	TIME	STIMULUS
			sec.	
Ye..........................	6	white	16	shock
Ye..........................	7	joining	3	shock
Ye..........................	8	white	5	shock
Ye..........................	9	hitew	14	shock
Ye..........................	10	white	3	shock

CONCLUSIONS: While this is not an entirely satisfactory method of studying the distance perception of the mouse, it makes possible some inferences that are of value. The objections which may be made to the method are that the activity of the animal on the disk, particularly the going round and round while hanging over

the edge, is not a search for the nearest point of the surface below, but merely the activity resulting from a kind of agoraphobia; and that the slight height of the disk from which the animal would not jump at all is due, not to any visual short-sightedness, but to the fear of jumping in general. Undoubtedly both of these conditions may obtain in the mouse when on the disk, yet the decreasing series of times elapsing before jumping, as recorded, corresponds in such a way with the decreasing height of the disk as to make it clear that vision does come into play in the estimating of distance. That this perception is not due to some other sense which receives the vibrations from near objects is shown by the poor results when glass was used. The animal seems to be unable either to see the glass or to be affected by any object on the other side of it, except when the object is very close to the glass. This is what we should expect, considering the apparently short range of vision of the mouse and its lack of perception of sharp visual outlines. The shiny surface of the glass is evidently connected with the estimated distance of objects on the other side. The black paper when covered with the glass appeared more glossy than the white. This doubtless made it appear less certain as a place to jump to than the white.

Turning the table, or the disk, or both, through an angle of 180 degrees, to act as a check against choice according to absolute position, changed the results only slightly.

PROBLEM 5. PERCEPTION OF THIRD DIMENSION IN OBJECTS

This problem is very nearly related to the preceding one. It has reference to the third dimensional nature of the object perceived rather than to the distance of the object from the animal. It is treated separately on account of the entirely different method and apparatus made use of.

APPARATUS: A box measuring 42 cm. x $23\frac{1}{2}$ cm. and 7 cm. deep, with openings at both ends, was used (fig. 7). At one end is attached the triangular enclosure *A*, and the other end of the box opens into the nest, *N*. Leading into the box from *A* is a narrow passageway 10 cm. long and 3.5 cm. wide. *B* and *C* are adjustable partitions which barely pass each other when moved back and

forth in the box. A scale is marked off on the floor of the box, extending from the point X at the end of the passageway to the opening into the nest.

METHOD: The animals are started from the enclosure A through the box toward the nest. Mice, owing to a stereotactic instinct, tend to linger in the passage and then dash quickly forth

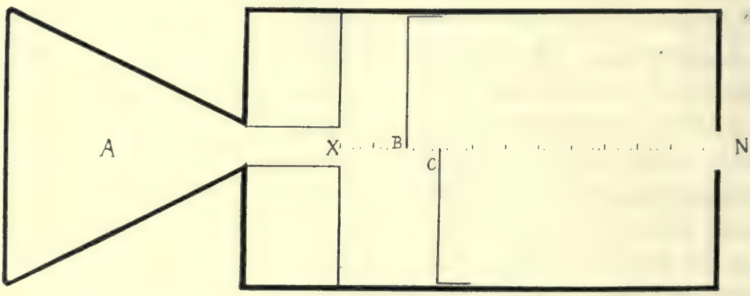


FIG. 7 Apparatus for the study of perception of depth (third dimension). A, triangular entrance chamber; X, passageway; B, C, moveable board partitions; N, nest-box.

across the open space toward the nest. In the experiment, therefore, the animal must observe from the point X which of the two partitions, B or C, is the nearer. If the partition on the left is nearer the animal, it is evident that, in order to pass through, he must swerve to the right as he runs forward, and then turn sharply to the left. If the partition on the right is the nearer, a manœuvre of the opposite sort is necessary. The mice should turn correctly in case they have perceived from X the true relative positions of the two partitions.

In the experiments food was scattered in the enclosure and the animals were allowed to run back and forth between it and the nest a few times, then while an animal was in A, the partitions were changed and a sharp clap of the hands sent him toward the nest. His behavior at the point X and at B or C was noted with care.

Thirteen mice were used.

MOUSE	COLOR	SEX	AGE	DATE OF EXPERIMENT
Ke	white	male	6 weeks	March, 1906
Kh.	white	female	6 weeks	March, 1906
Ka	white	male	6 weeks	March, 1906
J	white	male	adult	March, 1906
Q	white	male	adult	March, 1906
T	gray	male	3 months	Oct. 10
X	black	female	3 months	Oct. 15
BF	black	female	3 months	Oct. 15–17
U	black	male	5 weeks	Oct. 20–25, 1906
Xa	gray and white[1]	male	2 months	Feb. 25–Mar. 23, 1907
Xc	gray	male	2 months	Feb. 25–Mar. 23, 1907
Xd	black	female	2 months	Fed. 25–Mar. 23, 1907
Xe	white	female	2 months	Feb. 25–Mar. 23, 1907

[1] The gray and white mouse had pigmented eyes.

The animals were tried separately. After each trial the positions of the partitions B and C were reversed. The reaction was recorded as an error of choice, and therefore indicative of lack of perception of depth, if the mouse inclined to the left when B was the nearer to X, or to the right when C was the nearer.

As a rule the mistakes were very marked. Often an animal would run forward until its nose touched the partition. Again an individual would slowly approach the partitions, examine them at close range, and then dash through between them. Such reactions as this last were of doubtful value in connection with the problem.

RESULTS: The following tables give the results for different groups of mice and different positions of the partitions.

Albino mice. Partitions respectively 10 cm. and 15 cm. from X. Reversed after each trial

MOUSE	NO. TRIALS	RIGHT	WRONG	DOUBTFUL
Ke	10	6	4	–
Kh	36	14	16	6
Ka	11	6	4	1
J	20	11	8	1
C	10	8	2	–

Mice with black eyes. Partitions same distance from X; changed at irregular intervals, usually after two or three trials.

MOUSE	NO. TRIALS	RIGHT	WRONG
T	10	7	3
X	14	7	7
BF	26	18	8
U	10	3	7

In the next experiment the mice T, X, BF, and U were allowed to run back and forth through the box all night. The following morning while they were in the enclosure *A*, the partitions were interchanged and they were allowed to run through the box one at a time. They were then taken back and the process was repeated. In the eight trials, seven were wrong and one right. The same thing tried on a subsequent morning resulted in eight wrong choices.

Partitions nearer to the animal, but at the same distance from each other (5 cm.) One at 5 cm. the other at 10 cm. from X

MOUSE	NO. TRIALS	RIGHT	WRONG	DOUBTFUL
Xc	20	4	10	6
Xa	14	7	5	2
Xd	18	5	11	2
Xe	15	5	8	2

One partition 5 cm. from the passageway, the other 15 cm., making a depth of 10 cm. to be perceived

MOUSE	NO. TRIALS	RIGHT	WRONG
Xc	6	1	5
Xa	6	2	4
Xd	6	2	4
Xe	8	3	5

In all of the white mice there was observed a peculiar head movement from side to side while crouching in the passageway, preparatory to running forward. Since the results in the matter of depth-perception favor the white mice—for the black-eyed

mice the number of right choices being 47 per cent of the number of trials, while for the albino mice it is 53 per cent— it was thought that this head movement might be serviceable to the animals, as giving several points of view of the object, each from a different angle, thus possibly rendering a perception of depth easier.

Following this idea, a record was kept of the number of times the head movement was observed and this was compared with the right choices. I present as typical the results in the case of one mouse, Kh. This animal made 14 right choices and 16 wrong. Of the right choices the head movement was observed in 8 and not observed in 6, while of the wrong choices there was head movement in 1 and no head movement in 15.

CONCLUSIONS: Judging by the number of errors, we may conclude that the mice do not make use of visual perception of depth. If they have the anatomical equipment necessary for the perception of depth, their important muscular sense controls their actions, making them take the same course they took on the preceding occasion.

II. BINOCULAR VISION

The question of binocular vision in the mouse suggested itself in connection with the investigation into the perception of depth, and an attempt was made to find how far the structural conditions are fulfilled which would make it possible.

The conditions which must exist in order that binocular vision in the psychological sense may be present are:

(1) It is necessary that the eyes be so situated in the head as to have a portion of the field of vision common to each.

(2) There must be consensual movements of the eyes. The lines of sight of the two eyes must be capable of moving approximately parallel to one another so that the images of an object may fall on corresponding points of the two retinae.

(3) There must be a chiasm of the optic nerve and a portion of the fibres from one eye must mingle with a portion of those from the other, that is, there must not be a total decussation of optic fibres at the chiasm.

If the animal possesses the requirements estimated above, there is good reason to suppose that it has binocular vision in the proper sense.

A piece of anatomical work undertaken to supplement the experiments on behavior was that of measuring the angle of divergence of the optic axes, and determining the angular field of possible binocular vision. The results appear in fig. 8. In a head from which the eyes had been removed the optical axis was obtained by drawing a line from the fundus of the eye-socket through

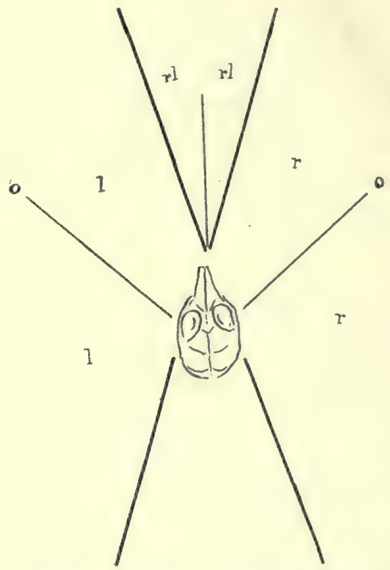


Fig. 8 The field of vision of a mouse. *r*, right monocular field; *l*, left monocular field, *rl*, binocular field; *o*, *o*, optic axes.

a point equally distant from all points in the rim of the opening, outward into space. The limits of the field of vision were determined by the points from which the eye could be plainly seen.

Although the axes of the conical eye-sockets in the mouse diverge greatly, forming an angle of about 100 degrees, yet, owing to the prominence of the eyes themselves, it is quite possible that they may receive images from the same object simultaneously.

I experimented by standing in front of the mouse where, with one eye closed, I could plainly see both eyes of the animal, and then moved my head from side to side in order to discover how great was the angular field from which the two eyes might be seen. I found that I could move my head through an angle of 70 degrees without losing sight of either eye.

Although the possibility of seeing the ball of the eye in a human being does not always mean that the eye sees the observer, yet we know that when the eye is turned as far as it can go toward the observer, he is seen on the extreme border of the field of vision.

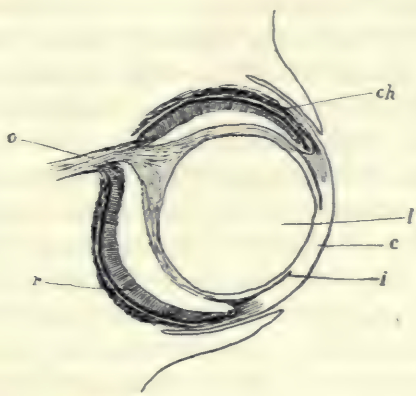


FIG. 9 Diagram showing relative size of lens in eye of mouse. *ch*, choroid; *l*, lens; *c*, cornea; *i*, iris; *o*, optic nerve; *r*, retina.

This of course, is made possible by means of the refractive power of the lens and by the fact that the retina extends all the way around the inside of the eye right up to the ciliary process.

If the retina were of uniform acuteness throughout its area, or if the lens were larger, it would not be necessary to move the eye to its limit in order to see the observer who can just see the eyeball. These are just the conditions which we have good reason to suppose exist in the mouse. In the first place the size of lens,

compared with the size of the eye, is much larger than in man (see fig. 9); and, second, the homogeneity of the retina is demonstrated in a later section (IV) of this paper.

From these considerations we feel justified in concluding that there is in the mouse a portion of the field of view which is shared by the two eyes.

Concerning the second requirement for binocular vision it is exceedingly difficult to secure data from observation of the animal. The uniform appearance of the surface of the eye makes it almost impossible to detect movements of the eye. The black glistening beadiness of the eye is always the same.[1] Several attempts were made to obtain a point of reference on the eye-ball by using a small square of paraffined Chinese white, but without success. The animal would close the eye immediately and dislodge it. Finally on close observation under a very bright light a faint line of the pupil could be distinguished, and when the light struck the eye at a certain angle, its movements could be observed.

The animal was held on the palm of the hand facing the observer and trials were made by moving a finger back and forth on different sides to learn the nature of the eye movements. When the finger was brought slowly above the level of the palm, within the field of vision for one eye, the mouse would turn its head slightly toward the finger. When the finger was moved to and fro, it was not followed by the eyes. Under these conditions the animal always reacted by remaining perfectly still with the eyes in a fixed position. A more rapid movement of the finger would elicit only a slight further turning of the head in the direction of the movement. The finger was then lowered and raised on the other side of the animal's head. These conditions were alternated and repeated about twenty times. The reaction was in all cases the same.

Fear cannot reasonably be suggested as a cause for the movements not being followed with the eyes, for the animal was perfectly tame. It was used to being handled daily and would run to

[1] It might naturally be supposed that the uniform beady appearance of the eye is of biological value in permitting the animal to make movements of the eye which, while enabling it to see its enemies, would themselves remain undetected.

my hand at any time. Further, the fact that the head was moved would make the view that fear inhibited the eye-movements untenable.

Such slight eye-movements as do occur seem to be rather for the purpose of getting more light from the general direction of the object than for getting a clearer image. There appears to be nothing in the vision of the mouse which compares with fixation in the human being. If the image of the object is cast upon any part of the retina, all the conditions are fulfilled which make vision useful to the mouse as a protective sense.

On the third requirement for binocular vision, the dividing and crossing of the optic fibres, positive statements can not be made from a mere study of the anatomical structure. The chiasm exists in all vertebrates, while in mammals, birds and to some extent, in reptiles the nerves unite, so that each is made up of fibres from both roots. In fishes they cross without uniting. Whether fibres from corresponding points in the retinae unite so that their excitations are carried to the same neural channels in the brain is practically undiscoverable, but the fact that there is any union at all rather than total decussation, indicates an intimate degree of relation between the two eyes.

Harris,[2] who worked by the method of degeneration upon the optic fibres of the lizard, ventures the statement that decussation in rats and mice is complete. More satisfactory knowledge on this point would be gained from the cumulative evidence of the degeneration method and the method of myelogenesis as used by von Gudden. The amount of mingling of optic fibres would not be great considering the fact that only small portions of the retinae can receive images from the same object. Cajal[3] working by the method of degeneration upon the optic chiasm of the rabbit and the mouse, has in fact shown that the crossing of the fibres is not complete.

[2] Harris, W. Binocular and stereoscopic vision in man and the other vertebrates, with its relation to the decussation of the optic nerves, the ocular movements and the pupil light-reflex. *Brain*, part cv, p. 107. 1904.

[3] Ramón y Cajal, S. Textura del sistema nervioso del hombre y de los verdebrados, vol. 2, p. 652, *Madrid*, 1904.

If there were no mingling at all of optic fibres, then we should be urged to the absurd conclusion that a single object which casts double images in the eyes is not interpreted as one, or else that there is an alternation of attention, a sort of psychical rivalry in which the sensation from one eye intermittently inhibits that from the other, a view which is not in accordance with the law of parsimony and is most improbable. As a third possibility it might be claimed that one of the images becomes the dominating stimulus while the sensation from the other is entirely inhibited. We find such a possibility proposed as a theory by Morgan.[4] He supposes, in the case of animals like the rabbit, where the eyes are so situated that they cannot combine in binocular vision, that "the image that falls on the most sensitive area or yellow spot of one eye suggests the focal impression, while that which falls on a similar spot in the other eye is marginal to its conscious consentience." The existence of such a yellow spot Morgan assumes. The need of a theory of this sort might be vindicated, were it shown that animals whose eyes diverge at so great an angle possess a fovea. Schäfer[5] states that only man and some primates have optic axes capable of convergence and a single central fovea.

The theory proposed above would be adequate for the explanation of the conditions which obtain in those animals, which in attending to an object turn one side of the head toward it, thus inhibiting any sensation from it by way of the other eye by practically excluding it from the other visual field, as do most birds. The mouse, however, turns toward an object enough for it to be clearly perceivable that lines from the object strike both eyes with a generous margin.

In reasoning from binocular vision in man to that possible in the mouse extreme caution is necessary, because in man many of the phenomena of binocular vision are closely connected with the central point of most acute vision in each retina. Convergence is meaningless unless we have reference to some definite point in

[4] Morgan, C. Lloyd. Introduction to Comparative Psychology. chap. 10, p. 160, *London,* 1902.

[5] Schäfer, A. E. Text-book of Physiology. vol. 2, p. 1148, 1900.

the retina, where the lines of sight which converge on the object terminate.

Although it is true that corresponding points function most accurately in the region of the central spot, as is shown by the difficulties attending the experimental determination of the horopter, yet it need not be true that they owe their existence to the presence of a central spot. The existence of corresponding points is quite possible in a retina, all portions of which are similarly organized.

The mouse, which has no fovea, might have certain portions of the eye adapted for binocular vision. These are the extreme posterior areas of the retina, which correspond to the temporal segments in the human being. It is here that images from one object can fall on both retinae and, therefore, here corresponding points must have been developed, otherwise the animal would perceive objects double.

The conditions here differ from those obtaining in the human being only in degree. In the latter a considerable portion of the nasal area of each retina can not function in binocular vision on account of the prominence of the bridge of the nose, and therefore a point in this region can have no point in the temporal portion of the other retina corresponding to it. In the mouse this area is much more extensive. If, for convenience, we call this area the monocular area, as distinguished from that in which corresponding points exist—the binocular area, then in an animal like the mouse the centre of the retina lies in this monocular area.

If there were a point of clearest vision near the center of the eye, it would be merely a fixation point which might function in lenticular accommodation. However there is no structural sign of a fovea in this region and obviously such a point in the monocular area could not function in convergence. We must conclude that the optical axis of the mouse's eye has no functional significance.

If we expected to find a fovea which serves the mouse as ours serves us, we would look for it in the extreme posterior portion of the retina, in the binocular area.

I made a number of observations upon squirrels in connection with this problem. In the squirrel the position of the eyes is some-

what less favorable to binocular vision than in the mouse. The snout of the mouse is proportionately longer than that of the squirrel, but the bridge between the eyes is so much lower that there is a large field of vision for two eyes above the head, which the squirrel does not possess.

When the squirrel under observation was approached from the side, he would sit on his haunches, lift up his head and show all signs of attention. When I would kneel on the ground within six feet of him and make no movement, he would remain with one side of his head toward me, using only one eye. When a movement was made by waving the hand back and forth, he would turn his head directly toward me in a position where both eyes were equally visible. This is a reaction very similar to that in the mouse.

Two explanations of this reaction suggest themselves: (1) The movement is made for the purpose of getting perspective which would aid in the perception of distance. Here convergence would be implied in the way that it occurs in human beings. (2) The head movement is made for purposes of orientation preparatory to turning the body in the direction of the stimulus and, perhaps, approaching it. The forward movements and turnings of the animal are executed with reference to a median plane, to which the precise relation of an object in space is more easily determined when the object is seen with both eyes. For determining the space relation of two objects both in the median plane, the one factor of location of images on the retina is adequate. Obviously a crawling animal like the mouse is more concerned with accuracy in its right and left movements than in movements in the vertical plane.

Of the two explanations given the latter is the more likely to prove correct. It is in accord with our experimental results in depth perception in the mouse, and further, the idea of convergence is not entirely compatible with a retina without a fovea, homogeneous throughout and of which the habits of the animal would demand only the function of communicating in a rough way the general nature of the object and its direction.

Perception of distance adequate to the animal's needs may be obtained through a synthetic correlation of retinal impressions and motor impulses of monocular accommodation.

Loeb[6] suggests that some animals may localize by means of changes in the form of objects, which must result from a marked astigmatism existing in the eyes of certain animals.

Cole[7] distinguishes four types of animal response to photic stimuli:

A. Response of eyeless forms.

B. Response of forms with direction eyes.

C. Response to size of luminous field.

D. Response to different objects in the visual field.

For our purpose we would make five types, dividing the last into two. Of these the first is to be found in those animals which perceive the presence of objects and a few general characteristics concerning them. The other is in those animals which have a distinct perception of form by means of a fovea or central spot of most acute vision. The mouse would be included under the first of these two. Under the latter would come man and the rest of the primates and some other animals such as the chameleon and certain birds of prey.

Animals of the former class, destitute of a fovea, although they may have a more delicate perception of the existence of objects in the field of view than we, yet do not see the form of objects regarded, as distinctly as we do.

A faint star is best seen with averted gaze owing to the fact that the rods are functioning and are susceptible to faint light stimuli but not to distinctness of outline. This is the case with the mouse, in whose retina cones do not exist.

The visual conditions existing in the mouse as revealed by our study of it are in accord with a view expressed by LeConte:[8] "In lower animals, especially those which are preyed upon by others, it is far more important to see well in every direction than to fix attention exclusively on one point, therefore, the advantages of exquisite microscopic distinctness of the center of the field are sacrificed for the much greater advantages of moderate distinct-

[6] Loeb, Jacques. *Arch. f. d. ges. Physiol.*, vol. 41, p. 371.

[7] Cole, L. J. An experimental study of the image-forming powers of various types of eyes. *Proc. Amer. Acad. Arts and Sciences*, vol. 42, p. 410, 1907.

[8] LeConte, J. Sight, chap. 5, *New York*, 1881.

ness over a very wide field. The most important thing for them is a very wide field and the equal distribution of attention over every part. Hence their eyes are prominent and destitute of a central spot so that they see all parts with equal distinctness."

III. KINAESTHETIC SENSATIONS, THE GUIDE TO MOVEMENT

In the human being who is at all introspective, kinaesthetic sensations often come into the focus of consciousness, but presumably more often they do not. In the latter case, strictly speaking, they are hardly to be called sensations. They exist merely as neural modifications. Traces are retained within the nervous structure in the form of facilitated transitions across the synapses, or, of increased permeability of the neurones. These neural processes operate in controlling the actions of the body without necessarily involving consciousness at the time. Sometimes they emerge into consciousness late, as when we become aware of having had our limbs in a certain position and know that they are not now in that position. Again we may have kinaesthetic images of bodily actions we are about to perform.

Our theory of the guiding sense in the mouse may be introduced with an illustration from human psychology, the phenomenon of alternating personalities. The normal person may do many things of which he is wholly unconscious, *e.g.*, he may lay an object in a certain place and, after a while, search for it, entirely unconscious of having put it anywhere himself. Later when another personality is dominant, it may occur that the knowledge of the location of the object is present to consciousness and there is no difficulty whatever in finding it. The second personality remembers putting it in the place in which it is found. This phenomenon may be explained on the supposition that during the incumbency of the former personality, the kinaesthetic sensations from the movements of the limbs are unable to emerge into the conscious field because other psychical processes, viz., those to which the normal person is attending, have control of the system of neurones whose excitement is accompanied by consciousness. Association of the kinaesthetic sensations in question with the present perceptions is

inhibited by the draining of the nervous energy of the association neurones of the higher senses in the direction of the frontal tracts of the brain. Now under these circumstances, it is evident, something must become of the nervous currents coming from the muscular sense organ. They seize upon certain motor neurones of the Rolandic area whose connection with the systems operating to reinforce the higher functions is not so direct, and consequently are not so thoroughly drained of energy. Such systems are mainly those which have functioned in bringing about the movements which gave rise to the kinaesthetic sensations. In them the resistance is low. Thus it comes about that motor circuits of the second level are formed. The passing of the synapses in these circuits is rendered progressively easier on account of the reverberating of impulses through the kinaesthetic-motor system. This system and the higher systems involving attention are for the time being mutually inhibitive.

The animal in choosing between alternatives is guided mainly by kinaesthetic sensations which have been registered in the nervous system, just as is the case with the subconscious personality. When the animal enters a compartment where it must choose between situation A on the right and situation B on the left, two internal factors tend to determine action. One of these is visual, the other is kinaesthetic, Of these two the visual is the one emphasized by Thorndike in his experiments with cats. Observation indicates that in the mouse, the visual stimuli are not of so great importance in guiding the animal as the kinaesthetic. The latter is relied upon wherever possible. Smell seems to be the next in importance. These considerations prompt us to adopt a law of parsimony in studying the senses of the mouse: we are warranted in inferring a case of visual determination only when there is no possibility of the muscular sense being used for the discrimination in question. Training a mouse to discriminate always involves training him away from a reliance on the muscular sense. This law may be applied to the muscular sense and smell, or, to smell and vision. There is a suggestion here of a possible criterion for grading the intelligence of the animal series, viz., the relative importance of the various senses in directing movement.

When Thorndike put his cats into a cage, the process of learning to open the latch consisted in the gradual association of a certain movement with certain sense impressions under certain conditions of hunger. Under the instinctive excitement caused by the situation the cat makes many movements; those in each part of the cage are guided by the visual impressions of that part of the cage acting by way of the visual cortex. Each group of visual impressions would thereafter, in accordance with the law of neural habit, tend to lead to the same movements more readily than before. One of these impressions acquires increased intimacy of association with a certain movement more readily and certainly than the rest, viz., the visual impression made by that part of the cage in which the latch is situated, with that movement which results in the falling open of the door. The intimacy of this particular association is increased each time this particular movement is made, until, as the cat casts its eye over the cage, the visual impression of that part of the cage at once evokes the movement. The explanation which Thorndike gives as to why the association between the particular sensory path and the particular motor disposition becomes fixed while other possible motor dispositions do not become fixed, is, that the former association gets "stamped in" by the pleasure resulting from it, while the other is "stamped out" by the pain of failure.

The kinaesthetic sense undoubtedly has a tendency to determine the cat's behavior, but vision operates more quickly, for the cat directs its attention to the visual stimuli, rendering possible readier association between the object seen and the motor mechanism. The condition here differs only in degree, not in kind, from that obtaining in the mouse.

The relative importance of muscular association and visual association may be well shown by the analysis of the actions of a mouse used in problem 2, B. If we go on the assumption that the mouse's action was associated with the result of the action immediately preceding, then we divide the whole series of choices of the animal into two kinds of sequences: *position sequences*, in which the mouse turns to the same side, left or right, where it received food in the preceding trial, and avoids the side where it received

a shock, and *color sequences*, in which the animal goes to the color where it received food in the preceding trial and avoids the one where it received the shock. The turnings to left or right were recorded in all the experiments, and in the case of mouse Q, in 100 choices we have the following results:

Position sequences	69
Color sequences	53
Position sequences in opposition to color	26
Color sequences in opposition to position	10

We interpret the behavior of the animal thus: it enters the compartment where a choice is necessary. Its attention may be on the idea of the food which it expects to receive or upon the pain of the electric shock or anything else you please—if we admit the possibility of such attention in the mouse—but this attentive consciousness is not directive unless it is associated with the idea of moving toward the food or away from the shock, and not then unless this idea is accompanied by the actual movements, at least in their incipiency. To put it in physiological terms, there must be a connection between cortical centers for representation and the motor areas, and this must be sufficiently energized to drain the energy from the kinaesthetic system. This, as we have been led to conclude from observing the behavior of the animal, is not generally the case. The governing of movements is turned over to the motor circuits, and thus it happens that time after time the mouse runs into the same compartment, the compartment in which it may receive a shock, but still the compartment which it entered on previous occasions sufficiently numerous for a motor circuit of a certain degree of stability to become established.

The reverberating of excitation through the motor circuit in the animal may be likened to a fly-wheel, which carries the animal in one direction or another by its momentum. The cue for the revolving of the wheel is afforded by the sensations which are constantly coming from the incipient movements of the animal. These may be reinforced by tactual impressions received from the floor and walls of the compartment.

The mental state of the mouse on entering the puzzle box may

be conceived as similar to that in which one finds one's self when learning to operate some little mechanical device:

I go to my locker after a long absence, having forgotten the combination, and as soon as the muscular sensations come in from handling the lock, I find that I am turning the knob to the particular succession of numbers which will open the lock. I become aware of the succession and by attending to the movements I am making, learn the combination from myself. In this case I have been observing the effects of the operation of a motor circuit.

Supposing I am not particularly anxious to relearn the combination; my attention is on the paraphernalia within the locker. It is possible that I may open the lock without learning the combination from myself and may come to the locker again the next day without an idea of the key. Instead of being wholly interested in what I seek, it is only when I fix my attention on the means of attaining it that I am in a position to learn; but I can accomplish my ends perfectly without paying attention to the means, letting the motor circuits do the work, and this is the way, we conceive, that the mouse does in the majority of cases where it is successful.

Now suppose that after I have become used to a certain combination and can work it unconsciously, the combination is changed and I am told a new one. I receive the new series of numbers and commit it to memory. The next day I go to the locker and find, after working with the knob for some time, that my fingers have been using the old combination. I may even make this same mistake for several days. This experience we may compare with that of the mouse when the lights are changed. The creature is guided by the effects of the motor circuits within its body to the right side, because it had become habituated to turning to the right when the food was to be obtained on that side. Now that the food is to be obtained on the left, it still goes to the right, and does so over and over again. Each attempt of this sort is of course recorded as a failure.

How long the animal will continue to run into the wrong compartment is a question of how long before it will begin to attend to the means to be employed in obtaining the food, or, it is a question of how long before the impulse to venture in search of food

will be overcome by the impulse to avoid the unpleasantness of the shock which ensues in case of failure, so that the animal becomes unwilling to venture at all.

In the case of the human being, if several repetitions are necessary to bring into the focus of consciousness the movements that that being is making, then we must not expect much of the mouse, and we must believe that a large number of the cases recorded as failures were not necessarily failures to discriminate between stimuli but, rather, secondarily automatic movements.

When a visual stimulus succeeds in calling forth a change in mode of action as its response—thus overcoming the kinaesthetic influences, which would tend to bring about a former mode of action—there is involved a discharge of energy through the higher association tracts and a conscious accompaniment. The law of parsimony proposed at the beginning of this section, and also our general observations, forbid us to assume that such conscious accompaniment is involved, but rather make for the view that the mental processes of the animal rise into consciousness of its movements at intervals not so frequent as in the human being, and that the kinaesthetic sense is the predominant directive sense in the mouse.

IV. STRUCTURE OF THE EYE OF THE MOUSE

The animals used for the anatomical study of the eye were from among those that had been used in the experiments. Some gray mice were chosen and some albinos; also a few dancing mice were included among those studied.

Method: The heads of mice that had recently died or had been killed were preserved in formol-alcohol. When ready to work with them, the eyes were removed and put through alcohols of increasing strength, for about an hour in each, up to 100 per cent. They were then transferred to xylol where they cleared over night and the next day they were put in soft paraffin for an hour. They were next placed in hard paraffin for a half hour and later imbedded in this same paraffin. Care was taken in the imbedding

that the exact orientation of the eye, dorsal-ventral and anterior-posterior, should be known and preserved as the paraffin hardened.

In those eyes to be used for the study of the retina it was found necessary to remove the lens because, being made harder by the alcohol than the rest of the eye, the knife in striking it would have a tendency to tear the more delicate retina. The lenses were removed best after the eye was in the hard paraffin. A knife was passed through the block just cutting off the cornea, the lens was then easily picked out with a needle and the whole block was reimbedded. In other eyes the lenses were removed while still in xylol.

The paraffin blocks were next cut by the microtome into sections ten micra in thickness. These were taken in serial order and mounted by the water method upon slides smeared with albumen. The slides were warmed, washed in xylol and plunged into absolute alcohol, after which they were run through alcohols of decreasing strength in order to use an aqueous stain. They were then stained in Delafield's hematoxylin for three hours, washed in water, then counterstained over night in orange-G solution. After having been removed from the last solution they were run up through the alcohols again, immersed in xylol and finally mounted in Canada balsam.

RESULTS: A diligent study was made of each series to learn the nature of the retinal elements. There was no evidence of cones to be found, either in those sections which had been cut from the dorsal side downwards, exhibiting horizontal sections of the retina at the middle of the eye, or in the first sections of the eye, where the elements would appear in cross section (fig. 10).

No cones were visible among the retinal elements in any section of the eye. There were among the rods certain larger appearances which were at first thought to be cones, but it was found that these were caused by the overlapping of some of the rods or by their separation from one another by an interval a little greater than usual. They were not cones, as they did not take any stain and in some cases they tapered away at both ends.

Some of the eyes were cut beginning with the fundus outwards. In the first few sections of the series cut in this way, where one

would expect to find cones if anywhere, there was no sign of a cross section of a cone.

The best presentation of the retina of the mouse can be given by a diagram (fig. 10). Fig 9 is a drawing of a section through the whole eye, showing the size of the lens in comparison with the rest of the eye.

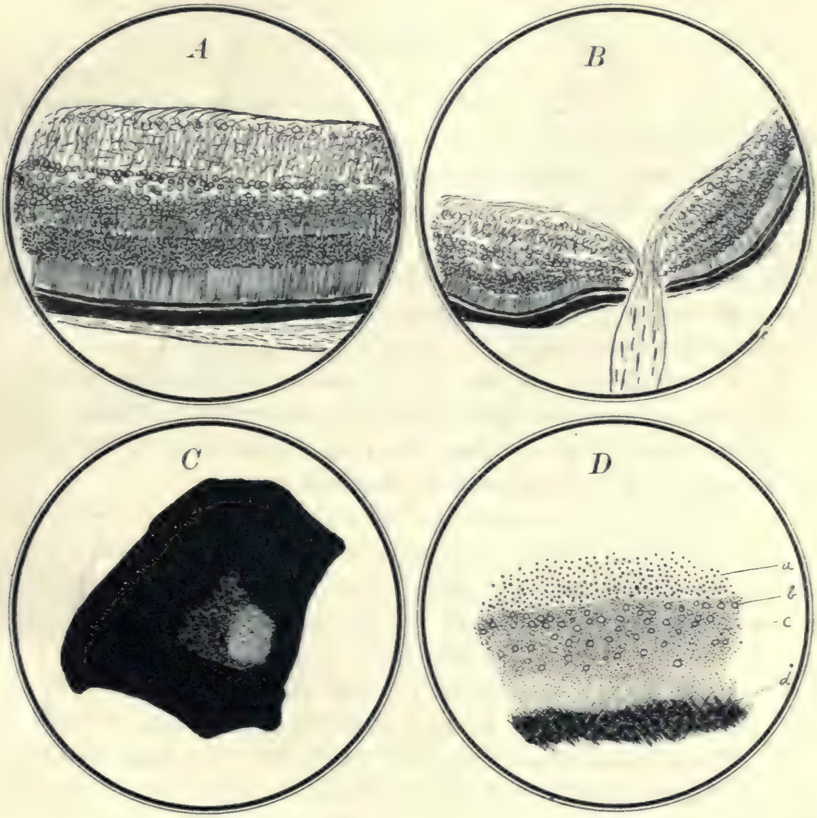


Fig. 10 A Section from central region of retina of mouse, showing rods without admixture of cones.

B Retina of mouse, showing entrance of optic nerve.

C First section of retina of eye of mouse, dorsal cutting, showing choroid coat, pigment cells and rods, no cones being present.

D Section of retina of eye of rabbit, showing rods and cones. *a*, nuclear layer; *b*, cones; *c*, rods; *d*, pigment.

A search through the sections of the eye failed to reveal structural signs of a fovea in any part of the eye. There was to be found no point in which the inner layers of the retina are sacrificed for the benefit of the rod layer, nor was there discovered a differentiation of the rods in any particular region.

V. SUMMARY

We may now bring together for convenience of reference the conclusions that have been drawn from the several researches:

1. The mouse distinguishes differences in grays and in brightness of lights with a considerable degree of accuracy. The discrimination of the albino mouse is not so good as that of the mouse with pigmented eyes. Illumination of the environment is influential in determining choice of light or dark objects. Black and white are preferred to grays.

2. Red and blue objects, which appear of the same intensity to the human eye, are discriminated between by the black mice, the percentage of error being less than in the case of the grays. Red and yellow are preferred to blue and green.

3. Albino mice do not show any discrimination between red and white lights. With black mice a very bright red and a white of low intensity are distinguished with greater difficulty than colors which are to the human eye of equal brightness. Discrimination between green and blue light is not apparent.

4. Perception of form is very poorly developed. The eye does not seem to be suited to any distinct perception of outlines.

5. The distance of objects is perceived within a range of 15 cm.

6. The mice fail to profit by estimating the depth of objects. The black mice make more errors in this respect than the albinos.

7. Our anatomical investigation shows mice to be lacking in retinal cones, confirming what has been surmised by Allen and by Morgan, and stated by Slonaker. We do not think it follows, as Morgan would suggest, that the absence of cones in mice, bats, hedgehogs and such nocturnal animals implies inability to distinguish colors. It is quite possible that the rods in the mouse are

adapted to the distinguishing of such few color contrasts as may be of importance in its life and habits.

8. There is no fovea or other structurally differentiated portion in the eye of the mouse. The range of vision is very wide, all parts of the retina being equally sensitive, a condition which is enjoyed at the sacrifice of distinct perception of form.

9. There is possible for the mouse a small field of binocular vision. This is not used for estimating distance, as there is no convergence of the eyes. It is of service rather as a means of orientation.

10. The kinaesthetic sense is more important than vision in determining the actions of the mouse. The latter is of use in indicating the presence and general direction of an enemy. Food is found largely through the sense of smell. In other words, smell is an active sense; vision is a protective or passive sense, while the behavior of the animal is largely the result of motor habits, formed through kinaesthetic sensations.

No. 14 332

THE INTELLIGENCE OF EARTHWORMS

ROBERT M. YERKES

From the Harvard Psychological Laboratory

Five figures

"But some degree of intelligence appears to be exhibited in this work [the plugging of the mouths of their burrows with various objects by earthworms],—a result which has surprised me more than anything else in regard to worms." [1] Thus wrote Charles Darwin as a result of his careful study of the habits of earthworms. Certain of his observations and the conclusion to which he felt himself forced have been discredited by the recent work of Miss Hanel.[2] Darwin, doubtless, was overliberal in his ascription of "mental qualities" to the worm, but Hanel certainly has proved herself extremely critical and parsimonious. In spite of the honest efforts of these excellent observers, and of many other biologists who have paid special attention to the behavior of earthworms, it must be admitted that we to-day know little concerning the possibilities of habit formation in these organisms.

The present investigation, taking its start from the work of Darwin, as critically repeated by Hanel, was planned for the purpose of (a) demonstrating the ability or inability of the earthworm to acquire direction-habits; (b) exhibiting the characteristics of such habits as might appear; (c) discovering external and internal factors important in connection with such habits; (d) determining the degree of permanency of habits; and (e) discovering the relation to the "brain" of such habits as appeared.

This paper is an introductory and preliminary account of an investigation which is still in progress. It is limited to a description of the general method employed and to a detailed account of the behavior of a single worm, No. 2, (*Allolobophora foetida*), which was under observation from October, 1911,

[1] Darwin, Charles. Formation of Vegetable Mould Through the Action of Worms. New York: D. Appleton and Company, 1898, p. 35.

[2] Hanel, Elise. Ein Beitrag zur "Psychologie" der Regenwürmer. Zeit. für allg. Physiol., 1904. Bd. 4, S. 244–58.

until September, 1912. It is the writer's purpose to present later a monographic account of his experiments with species of *Allolobophora* and *Lumbricus.*

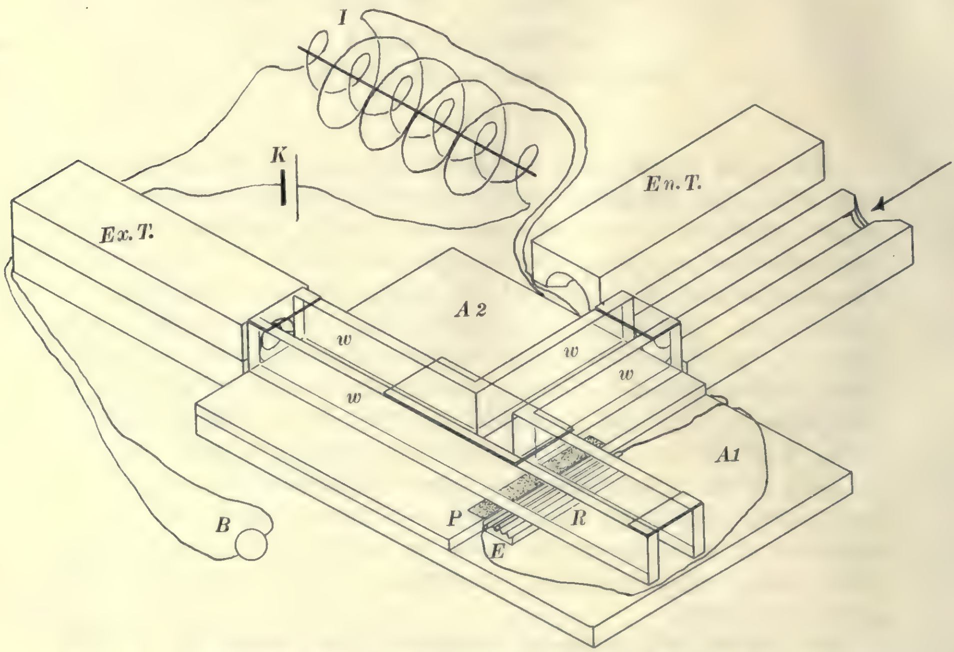


FIGURE 1. Perspective of T apparatus for study of habit formation in the earthworm. A 1, plate-glass base for parts of apparatus; A 2, layers of white blotting paper covering approximately two-thirds of A 1; w, w, w, w, plate-glass walls of T-shaped passage-way; En.T., wooden entrance tube, lined with moistened white blotting paper, from which worm enters passage-way, as indicated by arrow (the cover of the tube is shown removed); Ex.T., wooden exit tube in position for reception of worm as it emerges from open arm of glass T (in this case, the cover is in position); P, strip of sandpaper resting on A 2 and extending across passage-way; E, pieces of copper wire serving as electrodes, insulated and kept at fixed distance from one another by the corrugations of the strip of rubber, R; I, inductorium, wires from the secondary coil of which terminate in the electrodes at E; K, key in primary circuit of inductorium; B, dry cell.

In figure 1 is reproduced a perspective drawing of the apparatus used in the investigation. It consists of a T shaped labyrinth of plate glass, with runways 2 centimeters wide. The glass walls, w, are 2½ centimeters high and the distance from either entrance to the middle point of the unbroken wall is 12 centimeters. The walls are held together by four pieces of glass which are cemented to their upper surfaces with "cemen-

tium." The glass T rests upon a piece of plate glass two-thirds of which is covered, as is indicated in the figure, by layers of white blotting paper to the thickness of 5 millimeters The remainder of the base-plate is left uncovered in order that a piece of rubber, R, may be inserted under the T as insulation for two copper electrodes, E.

Wooden tubes which serve as artificial burrows appear at En.T. and Ex.T. The former may be designated the entrance tube and the latter the exit tube. The "tubes" are made by boring a 13 millimeter hole the length of a block of soft wood 14 by 4 by 4 centimeters. The block is then split. The ends of the hole are bevelled and sandpapered and the lower half is lined with white blotting paper.

A strip, 1 centimeter wide, of No. ½ sandpaper extends across one arm of the T, as shown at P, (figure 1); and just beyond the outer edge of the sandpaper, resting on the plate glass base, is the piece of corrugated rubber, R, in two of the grooves of which lie the copper electrodes, E.

In the early experiments with worm No. 2, a piece of glass carrying a strip of blotting paper which had been soaked in strong salt solution,[3] took the place of the rubber and the electrodes.

This apparatus was designed to test the ability of earthworms to "learn" to follow a simple path and to avoid an injurious chemical (or electrical) stimulus by reacting negatively to a peculiar tactual stimulus which regularly preceded the chemical. The experimenter had in mind two questions: First, can the worm profit by experience; second, can it "associate" the tactual stimulus with the chemical and acquire the habit of regularly responding to the sandpaper as tho it anticipated the effect of the salt.

In September, 1911, a number of manure worms were collected at Cambridge, Massachusetts, for this investigation. Those selected for use were kept in earthenware jars, 10 centimeters deep by 7.5 centimeters in diameter, in a moist mixture of earth and horse manure. Number 2, the subject of this report, was, from the first, an active, vigorous individual, which reacted well in the apparatus and evidently throve on the treat-

[3] The solution was made by dissolving 60 grams of c. p. sodium chloride in one liter of distilled water.

ment afforded it. Between September, 1911, and September, 1912, it doubled its size.

Each day, in preparation for the experiments with No. 2, the blotting paper upon which the glass T rested was soaked with tap water, as were also the paper linings of the entrance and exit tubes. The apparatus was then set with the stem of the T directed toward and perpendicular to a north window which was the sole source of illumination during experimentation. The strip of sandpaper now was properly placed beneath one arm of the T at 1 centimeter from the adjacent wall of the stem, and just beyond it, on the plate glass base, instead of on the blotting paper, a thin piece of glass supporting a strip of blotting paper soaked in salt solution was adjusted.[4]

When the apparatus was in readiness the experimenter removed the worm from its jar by inverting the latter over a dish and emptying the entire contents. Then with a fine camel's hair brush the worm was removed to a small dish of water, to wash off any adhering particles of earth. Thence it was carefully, but quickly, transferred to the lower half of the entrance tube and immediately covered with the upper half to protect it from the light.

A "trial" or "test" was given as follows: Having placed the entrance tube containing the worm at the base of the stem of the T, with the anterior end of the worm directed toward the T, the experimenter removed the cover of the tube, thus permitting the light from the window to drive the negatively phototactic worm into the T. If the light did not within a few seconds cause the subject to move forward, it was lightly stroked along the dorsum of the terminal segments with a moist camel's hair brush.

No. 2 usually started quickly, and especially in the later experiments, without being "touched."

The experimenter noted, by referring to a watch, the moment of uncovering the tube, and registered the beginning of the trip thru the T by starting a stop-watch the instant the head of the worm entered the stem of the T. He then observed minutely the behavior of the worm in the apparatus, recording as accurately as might be the path followed, the number of

[4] In the later experiments with worm No. 2 the strip of rubber and the electrodes took the place of the apparatus for chemical stimulation.

times the worm came into contact with the strip of sandpaper and the number of times it received the chemical (or electrical) stimulus.

The subject was permitted neither to retrace its course thru the stem of the T to the entrance tube, nor to escape by the arm containing the sandpaper.

The instant the "head" of the worm entered the exit tube the experimenter stopped the stop-watch and recorded the results of the trial.

For convenience, as well as accuracy, in the recording of

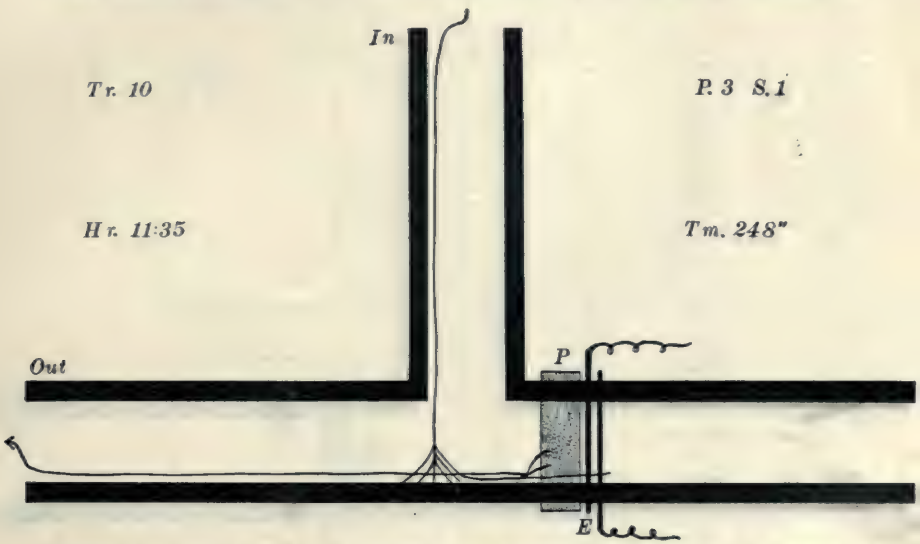


FIGURE 2. Ground plan of T, showing path followed by earthworm No. 2 in trial 10. (In this figure and the following one, fig. 3, a piece of blotting paper soaked in salt solution took the place of the electrodes at E.)

data an outline of the apparatus, similar to that of figure 2, was cut in rubber and on the blank impressions produced with this stamp the observer recorded (a) the number of the "trial," (b) the hour, (c) the total time for the "trip," (d) the number of contacts with the sandpaper, (e) the number of chemical (or electrical) stimulations, and (f) the path followed. Supplementary notes concerning features of the behavior frequently were appended.

Experimentation with worm No. 2 was begun October 12, 1911. On that day it was given ten trials in the apparatus. An

interval of about twenty minutes, gradually diminished to five in the later series, separated the trials.

For several weeks worms No. 2 and No. 1 were given trials alternately in order that they might obscure one another's mucous trails. From the first the sandpaper proved to be slightly repellent to No. 2. Consequently the chemical stimulus was received in only two of the first series of 10 trials. In accordance with chance, the worm turned as often to the right arm of the T as to the left arm.

Four days after the first series, on October 16, a second series

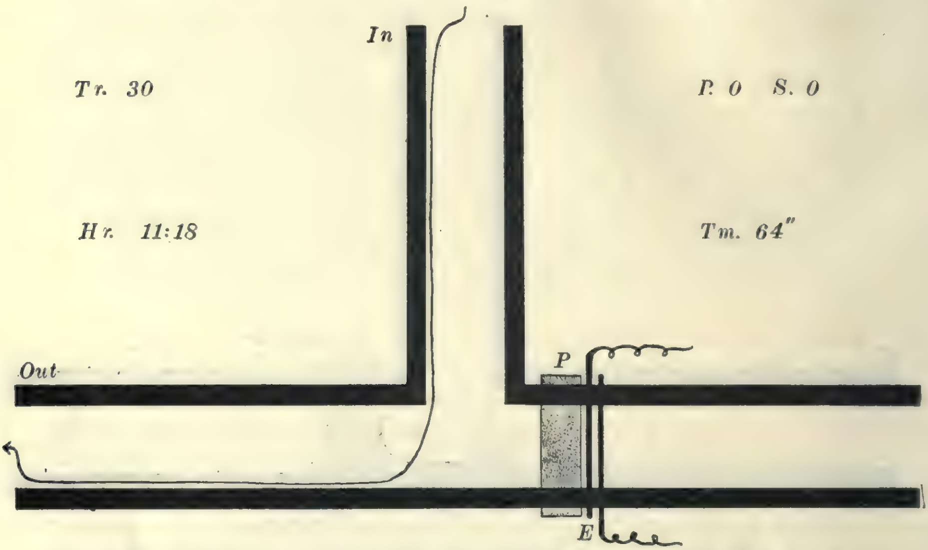


FIGURE 3. Diagram of T, showing path followed by earthworm No. 2 in trial 30.

yielded results strikingly similar to those of the first day, and as strikingly in contrast with those obtained on the third day.

The data for the first three series of trials are presented in table 1. In these results it is remarkable that, after only 20 experiences in the apparatus, No. 2 should have succeeded, on October 17, in passing thru the T from entrance tube to exit tube 10 times in succession without a mistake in the direction of turning and usually with directness.

In trial 21, the first on October 17, the worm followed the left wall of the stem of the T as far as the junction of the arms,

then turned sharply to the right and passed directly into the exit tube. In trials 22 to 30 it, instead, followed the right wall of the stem, thus taking the shortest route to the exit tube (figure 3).

The route shown in figure 2 is typical of the early indirect journeys; that of figure 3, of the later direct trips.

As appears in table 1, the average time for the first 10 trips was 163″; for the second 10, 103.5″; for the third 10, 68.5″.

TABLE 1

Results of First Thirty Training Experiments with Worm No. 2

Date	Trial	Contacts with Sandpaper	Contacts with Salt	Time
Oct. 12	1	0	0	61″
"	2	2	0	84″
"	3	0	0	68″
"	4	0	0	85″
"	5	3	0	146″
"	6	2	0	173″
"	7	12	5	480″
"	8	0	0	133″
"	9	0	0	152″
"	10	3	1	248″
Average of ten trials		**2.2**	**.6**	**163″**
Oct. 16	11	1	1	126″
"	12	1	0	66″
"	13	1	0	71″
"	14	0	0	60″
"	15	3	1	159″
"	16	0	0	87″
"	17	2	1	180″
"	18	0	0	63″
"	19	0	0	114″
"	20	2	0	109″
Average of ten trials		**1**	**.3**	**103.5″**
Oct. 17	21	0	0	75″
"	22	0	0	109″
"	23	0	0	58″
"	24	0	0	46″
"	25	0	0	63″
"	26	0	0	61″
"	27	0	0	70″
"	28	0	0	56″
"	29	0	0	83″
"	30	0	0	64″
Average of ten trials		**0**	**0**	**68.5″**

These results strongly suggest to the student of animal behavior "tracking" or the directive influence of light. To the

experimenter it seemed extremely improbable that the worm had acquired a perfect direction-habit as a result of only 20 trials in the T. For the present it must suffice to state that no satisfactory explanation for the correct series was revealed by check experiments. The worm apparently profited by experience with surprising quickness.

Between October 12 and November 6, as may be noted in the accompanying summary of experiments with No. 2, the worm was given 200 trials in series of 10 each. The results were variable. Thus we find that the perfect record for October 17 was followed by a record of contact with the sandpaper in three trials and with the salt in one trial on October 18, and that, in turn, by a perfect record on October 19.

SUMMARY OF EXPERIMENTS WITH WORM No. 2

Trials	Dates	Direction of Turning	No. of Trials	Stimuli	Condition of Worm
1- 200	Oct. 12-Nov. 6, 1911	Right	10	Paper and salt	Normal
201- 400	Nov. 7-Nov. 17, 1911	"	20	" "	"
401- 510	Nov. 20-Nov. 30, 1911	"	10	" "	"
511- 645	Dec. 2, 1911-Jan. 25, 1912	"	5, 10, 15	" "	"
646- 710	Jan. 26-Feb. 13, 1912	"	5	" and elect.	"
711- 780	Feb. 15-Apr. 9, 1912	Left	5	" "	"
781- 860	Apr. 10-Apr. 30, 1912	Right	5, 10	" "	"
861- 920	May 2-June 7, 1912	"	5	" "	"Brainless"
921-1000	July 4-July 25, 1912	"	5	" "	"New Brain"

The training on the basis of 10 trials per day (with omission of experimentation occasionally) failed to establish a definitely predictable mode of response (perfectly dependable habit) in No. 2, so the number of trials per series was increased to 20, beginning on November 7. Thus the experimentation was continued until November 17, when No. 2 was given its four-hundredth trial. The results varied extremely from day to day. There appeared to be "good" and "bad" days. The data of tables 2 and 3 are offered in support of this statement.

Thus, November 8 (table 2) proved to be a "bad" day as compared with November 9. Likewise, November 10 proved to be unfavorable, whereas November 11 was favorable (table 3).

TABLE 2

Results, with Worm No. 2, Indicative of Irregularity of Performance from Day to Day. ("Good" and "Bad" Days)

Date	Trial	Sand-paper	Salt	Time	Date	Trial	Sand-paper	Salt	Time
Nov. 8	221	0	0	110″	Nov. 9	241	4	3	204″
"	222	0	0	55″	"	242	0	0	60″
"	223	1	0	73″	"	243	0	0	59″
"	224	0	0	79″	"	244	0	0	65″
"	225	3	1	94″	"	245	2	0	104″
"	226	0	0	113″	"	246	0	0	81″
"	227	1	0	82″	"	247	0	0	68″
"	228	3	1	117″	"	248	0	0	65″
"	229	1	0	89″	"	249	0	0	51″
"	230	0	0	73″	"	250	0	0	59″
"	231	1	1	120″	"	251	0	0	62″
"	232	1	0	92″	"	252	0	0	65″
"	233	0	0	130″	"	253	0	0	58″
"	234	1	1	111″	"	254	0	0	80″
"	235	5	2	185″	"	255	2	0	81″
"	236	0	0	98″	"	256	2	0	122″
"	237	0	0	99″	"	257	2	0	100″
"	238	0	0	90″	"	258	0	0	64″
"	239	4	1	136″	"	259	0	0	157″
"	240	0	0	120″	"	260	1	0	73″
Av. of twenty trials		**1.05**	**.35**	**103.3″**	Av. of twenty trials		**.65**	**.15**	**83.9″**

TABLE 3

Results, with Worm No. 2, Indicative of Irregularity of Performance from Day to Day. ("Good" and "Bad" Days)

Date	Trial	Sand-paper	Salt	Time	Date	Trial	Sand-paper	Salt	Time
Nov. 10	261	5	0	162″	Nov. 11	281	5	0	154″
"	262	0	0	133″	"	282	0	0	72″
"	263	0	0	97″	"	283	0	0	69″
"	264	0	0	86″	"	284	0	0	82″
"	265	0	0	81″	"	285	1	0	90″
"	266	0	0	105″	"	286	1	0	107″
"	267	0	0	87″	"	287	1	0	59″
"	268	3	1	138″	"	288	0	0	50″
"	269	0	0	100″	"	289	0	0	46″
"	270	0	0	105″	"	290	0	0	47″
"	271	2	1	119″	"	291	0	0	54″
"	272	3	2	138″	"	292	2	0	109″
"	273	7	3	239″	"	293	0	0	61″
"	274	0	0	98″	"	294	0	0	49″
"	275	0	0	154″	"	295	0	0	50″
"	276	2	0	77″	"	296	0	0	48″
"	277	0	0	73″	"	297	0	0	41″
"	278	3	1	110″	"	298	0	0	45″
"	279	2	0	107″	"	299	0	0	61″
"	280	0	0	240″	"	300	0	0	59″
Av. of twenty trials		**1.35**	**.4**	**122.4″**	Av. of twenty trials		**.5**	**0**	**67.6″**

All things considered, the results of the training by 20 trials per day were less satisfactory than those of the earlier training by 10 trials. The worm frequently exhibited fatigue before the completion of a 20-trial series. Concluding that he had been overworking his subject, the experimenter reduced the number of trials to 10 per day from November 20 to November 30,—trials 401–510.

TABLE 4

Results of First Trials in Daily Series Compared with Averages for the First Half of the Trials of the Daily Series

Trial	Sandpaper	Salt	Time	Trials	Sandpaper	Salt	Time
1	0	0	61″	1– 5	1.0	0	88.8″
11	1	1	126″	11– 15	1.2	.4	96.4″
21	0	0	75″	21– 25	0	0	70.2″
31	0	0	65″	31– 35	2.2	1.4	142.8″
41	0	0	82″	41– 45	0	0	64.8″
51	6	5	125″	51– 55	2.2	1.4	130.4″
61	1	0	120″	61– 65	5.0	2.8	165.8″
71	1	0	150″	71– 75	.8	0	122.0″
81	2	1	124″	81– 85	3.8	1.4	197.0″
91	0	0	84″	91– 95	.4	.2	104.6″
101	3	2	228″	101–105	1.2	.8	158.0″
111	3	2	167″	111–115	1.6	1.0	121.8″
121	7	6	283″	121–125	1.6	1.2	123.8″
131	1	1	149″	131–135	1.6	.8	119.0″
141	15	11	345″	141–145	3.2	2.4	137.2″
151	1	1	150″	151–155	1.4	.4	123.2″
161	14	10	345″	161–165	3.2	2.0	131.6″
171	0	0	103″	171–175	2.0	.4	113.6″
181	0	0	92″	181–185	.4	.2	84.0″
191	4	1	158″	191–195	2.0	.6	112.2″
201	3	0	89″	201–210	1.2	.3	88.2″
221	0	0	110″	221–230	.9	.2	88.5″
241	4	3	204″	241–250	.6	.3	81.6″
261	5	0	162″	261–270	.8	.1	109.4″
281	5	0	154″	281–290	.8	0	77.6″
301	7	2	222″	301–310	1.2	.4	95.3″
321	1	0	102″	321–330	1.4	.3	102.2″
341	0	0	62″	341–350	.7	.5	100.1″
361	8	0	169″	361–370	2.2	.1	98.3″
381	3	1	187″	381–390	1.5	.5	109.8″
401	4	2	289″	401–405	1.8	1.0	156.2″
411	0	0	58″	411–415	0	0	72.0″
421	1	0	113″	421–425	.6	.2	134.0″
431	4	2	279″	431–435	.8	.4	112.8″
441	0	0	104″	441–445	1.0	.4	101.0″
451	3	1	213″	451–455	.8	.2	110.8″
461	0	0	206″	461–465	.6	.2	154.2″
471	0	0	151″	471–475	.6	.6	172.4″
481	0	0	77″	481–485	0	0	73.2″
491	3	1	67″	491–495	1.2	.4	114.4″
Averages	**2.75**	**1.32**	**151.2″**	Averages	**1.34**	**.59**	**114.0″**

Table 4 presents, on the left, the results of the first trial in each daily series up to the five-hundredth trial, and, on the

right, the averages of the data for the first half (5 or 10) of the trials of each daily series. It appears that more than twice as many contacts with sandpaper and salt were made in "first" trials as in the "first half" of the series. There is evidence of daily improvement in this comparison, but no proof of the acquisition of a habit which lasts from day to day.

The experimental procedure was further modified in the interval between December 2, 1911, and January 25, 1912,—trials 511 to 645—by the use of as few as five trials in a series, and by the frequent changing of the blotting paper under the T and the washing of the walls of the T in order that "tracking" should be rendered impossible.

The results demonstrated (a) the superiority of five trials per day over a greater number, and (b) the tendency of No. 2 to follow its own mucous path.

Repeatedly it happened that the worm after being permitted to make several direct trips thru the T, *without change of floor*, made mistakes, hesitated, and wandered as a result of the substitution of fresh blotting paper and the washing of the walls with tap water. But like the other features of the behavior of the worm, the following of the mucous trail laid down in the previous trip is not constant. In many instances the path is wholly disregarded. Only exceptionally, therefore, could a "perfect" series result from the "tracking" tendency. Moreover, since no systematic study of this tendency was made previous to the five-hundredth trial, it is not certain that No. 2 did not acquire its ability to follow its mucous trail. Of chief importance in the present connection is the fact that "tracking" does not suffice to account for the "perfect" series obtained.

On January 26 an important improvement in the technique of the experiment was made by the substituting of the electrical stimulus for the chemical. The salt solution had proved unsatisfactory because each time the worm came into contact with it a certain amount was converged to the adjacent blotting paper. The electrical stimulus proved to be cleaner, more controllable and therefore preferable.

The electrical stimulus consisted of the induced current from a Porter inductorium, with secondary set at 6-7 on the scale, in circuit with a No. 6 Columbia dry battery.

From January 26, 1912, till February 13,—trials 646 to 710—

series of five trials each were given. Only exceptionally did the worm receive the shock in other than the first trial of a series, and in several instances, the series were almost perfect.

At this point in the experimentation is was decided to attempt to break the habit of escaping from the T by the right arm and to substitute for it a habit of escaping by the other arm. Between February 15 and April 9,—trials 711 to 780—five trials per day were given with the sandpaper and electrodes in the right arm, instead of in the left, and with the exit tube at the end of the left arm.

Under these conditions the worm persistently turned to the right, in accordance with its previous training. So strong was this tendency that in the consecutive series of April 6, 7 and 9 not a single correct trial appeared (table 5, left side).

TABLE 5

RESULTS OF ATTEMPT TO BREAK UP HABIT OF TURNING TO RIGHT IN T. AND ESTABLISH THE HABIT OF TURNING TO THE LEFT

Turning to Left

Date	Trial	Sand-paper	Shock	Time
April 5	761	4	1	200″
"	762	0	0	53″
"	763	5	2	134″
"	764	4	2	260″
"	765	0	0	119″
Av. of five trials		**2.6**	**1.0**	**153.2″**
April 6	766	3	1	248″
"	767	6	3	193″
"	768	3	1	65″
"	769	2	1	107″
"	770	2	1	65″
Av. of five trials		**3.2**	**1.4**	**135.6″**
April 7	771	3	1	149″
"	772	3	3	172″
"	773	2	1	170″
"	774	2	3	145″
"	775	3	1	92″
Av. of five trials		**2.6**	**1.8**	**145.6″**
April 9	776	5	2	173″
"	777	6	1	490″
"	778	4	2	189″
"	779	3	1	151″
"	780	2	1	111″
Av. of five trials		**4.0**	**1.4**	**222.8″**

Turning to Right

Date	Trial	Sand-paper	Shock	Time
April 10	781	0	0	31″
"	782	1	1	67″
"	783	0	0	40″
"	784	0	0	58″
"	785	0	0	30″
"	786	0	0	39″
"	787	0	0	42″
"	788	0	0	44″
"	789	0	0	148″
"	790	0	0	54″
Av. of ten trials		**.1**	**.1**	**55.3″**
April 11	791	0	0	130″
"	792	0	0	54″
"	793	0	0	45″
"	794	0	0	57″
"	795	0	0	45″
Av. of five trials		**0**	**0**	**66.2″**
April 12	796	0	0	97″
"	797	0	0	28″
"	798	0	0	51″
"	799	0	0	36″
"	800	0	0	65″
Av. of five trials		**0**	**0**	**55.4″**

After 70 trials, this attempt to break the direction-habit

was given up because of the risk of injury to the subject from frequent electrical stimulation.

Beginning with April 10, the original conditions of training were reestablished, and, with few exceptions, the correct course was taken in trials 781 to 860. Several series were perfect.

The contrast between the features of behavior under the differing conditions of training—with apparatus arranged for escape by the left arm, and for escape by the right arm—is indicated in table 5. With the new condition of training there were few successful trials, with the old there were few failures. With the new, the worm wandered, baffled; with the old it escaped directly.

In the three weeks of training, April 10-30, the tactual and electrical stimuli were seldom received. The worm usually followed the right wall of the stem of the T from the entrance tube and turned to the right with a fair degree of directness upon coming in contact with the "common" wall of the arms.

The results of trials 781 to 860 were more clearly indicative of a definite direction-habit than any previously obtained. Table 6 presents a few typical series. Excellent evidences of the association of tactual with electrical sensations also appeared.

TABLE 6

RESULTS INDICATIVE OF PERFECTION OF HABIT BEFORE AMPUTATION OF FIRST FIVE SEGMENTS

Date	Trial	Sand-paper	Salt	Time	Date	Trial	Sand-paper	Salt	Time
April 23	801	0	0	52″	April 25	821	0	0	141″
"	802	0	0	76″	"	822	0	0	30″
"	803	0	0	45″	"	823	0	0	58″
"	804	0	0	36″	"	824	0	0	37″
"	805	0	0	53″	"	825	0	0	38″
"	806	0	0	40″	"	826	0	0	28″
"	807	0	0	73″	"	827	0	0	35″
"	808	2	0	78″	"	828	0	0	41″
"	809	0	0	49″	"	829	0	0	45″
"	810	3	0	240″	"	830	0	0	48″
Av. of ten trials		.5	0	74.2″	Av. of ten trials		0	0	50.1″
April 24	811	0	0	49″	April 26	831	0	0	61″
"	812	3	0	240″	"	832	3	0	63″
"	813	0	0	30″	"	833	0	0	52″
"	814	0	0	24″	"	834	0	0	49″
"	815	0	0	21″	"	835	0	0	43″
"	816	0	0	40″	"	836	0	0	54″
"	817	0	0	47″	"	837	0	0	72″
"	818	0	0	39″	"	838	0	0	43″
"	819	0	0	62″	"	839	0	0	89″
"	820	0	0	39″	"	840	0	0	281″
Av. of ten trials		.3	0	59.1″	Av. of ten trials		.3	0	80.7″

In view of this positive result of the training, it was deemed worth while to proceed with the next step in the investigation,—namely, amputation of the anterior segments, with the "brain," in order that the relation of the habit to the "brain" might be studied.

At 5 p. m. on April 30 the prostomium, the first four segments and about half of the fifth were amputated with a sharp pair of scissors. The worm was in splendid physical condition at the time. It reacted markedly for a few seconds but not violently to the mutilation. Immediately after the operation it was placed between layers of moist filter paper in a jar. Thus it was left over night.

When uncovered and exposed to the light at 10 a. m., May 1, it immediately began to crawl forward. It seemed very sensitive to photic and tactual stimuli, and it moved freely both forward and backward. The wound, which had closed, appeared to be in excellent condition. After the examination, the filter paper was removed, the jar washed and the worm replaced between layers of fresh paper.

So wholly satisfactory was the condition of the subject on May 2, 40 hours after the operation, that it seemed desirable to resume experimentation.

With the apparatus arranged precisely as in the previous series of trials, (April 30) the worm was permitted to enter the T. It moved forward, more slowly and continuously than before the operation, into the middle of the stem. Thence it proceeded by a path which is reproduced in figure 4. Having reached the "common" wall of the arms, it turned to the left and five times pushed forward to the sandpaper, each time withdrawing upon contact. As it searched with the cut end, for a way of escape, the "tail" became active and moved about as if "feeling" for a path. Shortly a turn toward the right was made and, with repeated attempts to crawl up the glass wall, the worm approached the exit tube. The instant the "head" end came in contact with the moist lining of the tube the worm pushed forward as in "recognition" of the retreat.

The trial which has been described was given at 9:16 a. m., and at 9:30 a second trial resulted in a direct trip thru the T The note-book record reads: "Entered quickly and directly. With 'head' end raised 3-4 millimeters from the floor and in

contact with the right wall of stem it rapidly moved forward to junction of arms. It then veered sharply toward the exit tube. On reaching the "common" wall it stopped and felt about for a few seconds, making no attempt to reach up or to swing from side to side. In this the behavior is unusual. After a few seconds it started for the tube, gradually ascending the wall of the arm as it approached the exit." The trip was a quick one, in which only the unusual features have been mentioned.

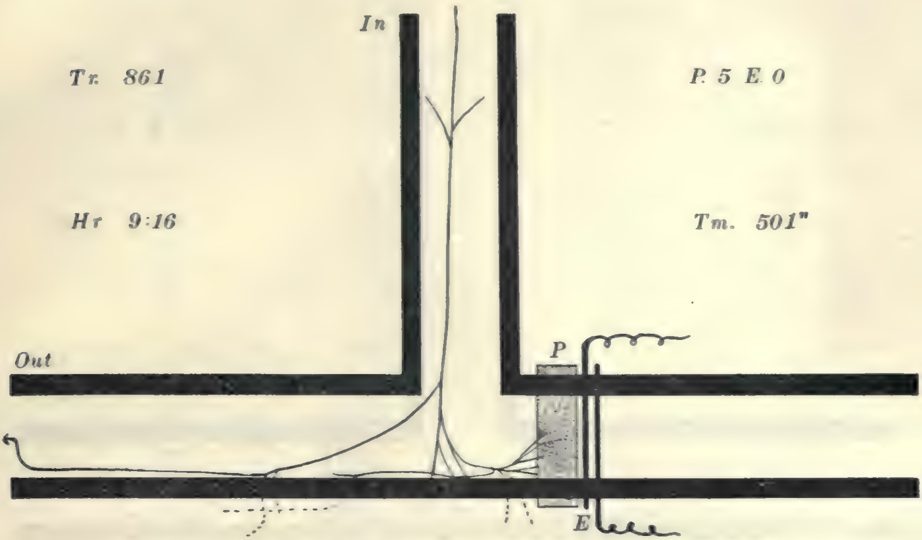


FIGURE 4. Diagram of T, showing path followed by earthworm No. 2 in first trial after removal of the brain.

The third trial, at 9:52, and the fourth, at 10:03, yielded results similar to that of the second. But in the fifth, 10:15, the path for which appears in figure 5, the worm followed the left wall of the stem instead of the right, felt about at the point of contact with the "common" wall, finally turned toward the exit, and, with frequent attempts to crawl over the wall of the arm, approached, reached, and quickly entered the tube.

Following this first series of trials with the "brainless" worm, at intervals of from two to seven days, additional series of five trials each were given until June 7. The general results appear in table 7.

The most important features of the behavior during this period of rapid regeneration of the "head" will now be described.

In trial 866 (first of May 4) the worm chose the wrong arm. Unmindful of the sandpaper, it pushed forward until the cut end touched the first electrode, whereupon it suddenly drew back and turned toward the exit tube. That this reaction was due to anticipation of the electrical stimulus is possible, but quite as likely it was due to a chemical stimulus from the copper electrode.

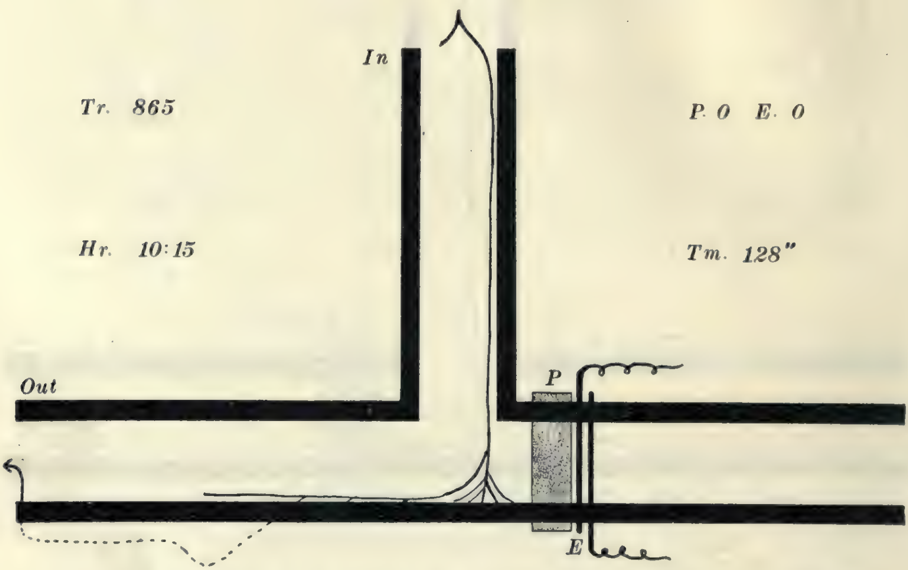


FIGURE 5. Diagram of T, showing path followed by earthworm No. 2 in fifth trial after removal of brain.

In the very next trial (867) the worm turned toward the right as it left the entrance tube, and, coming to the wall of the stem, pushed vigorously against it, while continuing to move forward, as if trying to round the corner of the stem and right arm before reaching it. To the experimenter, it looked as tho it anticipated the turn and miscalculated the distance by just about the length of the amputated segments. As soon as it reached the angle of stem and arm, it veered sharply to the right, while moving toward the "common" wall. Upon reaching the latter it pushed against it, stopped for a moment,

then turned to the right again and moved directly and rapidly into the tube.

TABLE 7

RESULTS OF EXPERIMENTS AFTER REMOVAL OF BRAIN ON APRIL 30

Date	Trial	Sand-paper	Shock	Time
May 2	861	5	0	501″
“	862	0	0	91″
“	863	0	0	82″
“	864	0	0	102″
“	865	0	0	128″
Av. of five trials		**1**	**0**	**180.8″**
May 4	866	2	0	403″
“	867	0	0	84″
“	868	0	0	201″
“	869	0	0	271″
“	870	1	0	310″
Av. of five trials		**.6**	**0**	**253.8″**
May 6	871	2	1	505″
“	872	0	0	74″
“	873	1	0	441″
“	874	0	0	130″
“	875	3	2	644″
Av. of five trials		**1.2**	**.6**	**358.8″**
May 9	876	0	0	149″
“	877	0	0	126″
“	878	0	0	465″
“	879	0	0	155″
“	880	0	0	231″
Av. of five trials		**0**	**0**	**225.2″**
May 13	891	0	0	273″
“	892	0	0	231″
“	893	0	0	107″
“	894	0	0	330″
“	895	0	0	386″
Av. of five trials		**0**	**0**	**265.4″**
May 21	896	1	0	327″
“	897	0	0	73″
“	898	0	0	55″
“	899	0	0	76″
“	900	1	0	117″
Av. of five trials		**.4**	**0**	**129.6″**
May 28	901	3	1	300″
“	902	0	0	72″
“	903	0	0	91″
“	904	0	0	48″
“	905	0	0	53″
Av. of five trials		**.6**	**2**	**112.8″**
May 29	906	4	2	355″
“	907	0	0	64″
“	908	0	0	57″
“	909	0	0	39″
“	910	0	0	60″
Av. of five trials		**.8**	**.4**	**115″**
June 6	911	0	0	430″
“	912	2	0	185″
“	913	0	0	77″
“	914	0	0	65″
“	915	0	0	54″
Av. of five trials		**.4**	**0**	**162.2″**
June 7	916	4	1	442″
“	917	0	0	52″
“	918	0	0	47″
“	919	0	0	45″
“	920	0	0	60″
Av. of five trials		**.8**	**.2**	**129.2″**

Trial 871, given at 10:45 on May 6, five and one-half days after the operation exhibited precisely the kind of behavior that the experimenter had expected of the "brainless" worm. The subject, with its conspicuous "plug" of regenerating tissue, moved forward readily, but reversed whenever an obstacle was encountered. It wandered about a great deal, as if lost. Finally, it touched the electrodes, received the shock, and reacted violently by "springing back" to the middle of the T. It then

reversed and *backed* into the exit tube. The posterior end seemed to "recognize" the tube. Thruout this trial the "tail" manifested unusual initiative.

The worm lost itself in the third, and again in the fifth trial. Concerning the latter the note-book affords the following comments. "The behavior contrasts markedly with that of previous trials. There is little initiative for forward movement. The worm finally had to be driven into the exit tube."

May 6 was evidently a "bad" day, due probably to an unfavorable physical condition.

The worm did not burrow in earth for three weeks after the operation. On May 21, it was found completely covered with loose earth. The regenerated portion at this time appeared clearly segmented and complete, but not yet as large as were the amputated segments.

The tendency to climb the walls of the T which was so conspicuous a feature of the behavior immediately after the operation gradually disappeared as the process of regeneration progressed. Within three weeks, the worm pushed along close to the floor of the apparatus, as it did before the operation.

For the first time, the worm was found naturally embedded in a well-formed earth-burrow on May 28. The regenerated segments were nearly full-size and differed from the others only in their lighter color. On this date the worm wandered more than formerly. It exhibited increased initiative and a proportional increase in mistakes. Whereas immediately after the operation the subject was automatic in its reaction to the apparatus, it now exhibited varied response.

From June 7 until July 4, a period of four weeks, the subject was permitted to rest in order that the persistence of the habit might be tested.

Previously the effect of rest had been observed by the suspension of training from December 19 until January 8, three weeks, with the result that the trials immediately after the "rest" were better than those before it. No indication of loss of the habit by the normal worm within three weeks appeared.

From the outset the trials with the regenerated worm, following upon a four-week's interval of rest, were marked by mistakes. On July 4 not a single trial was correct; on July 5, only two out of five; on July 8, only one of five (table 8).

TABLE 8

RESULTS OF EXPERIMENTS AFTER COMPLETE REGENERATION AND ONE MONTH WITHOUT TRIALS IN T

Operation, April 30; Last Trial, June 7

Date	Trial	Sand-paper	Shock	Time	Date	Trial	Sand-paper	Shock	Time
July 4	921	1	1	268″	July 16	961	0	0	38″
"	922	2	1	178″	"	962	0	0	44″
"	923	5	1	755″	"	963	1	1	65″
"	924	3	1	461″	"	964	0	0	46″
"	925	9	4	987″	"	965	0	0	53″
Av. of five trials		**4.**	**1.6**	**529.8″**	**Av. of five trials**		**.2**	**.2**	**49.2″**
July 5	926	0	0	113″	July 17	966	3	0	71″
"	927	0	0	54″	"	967	0	0	35″
"	928	3	0	68″	"	968	0	0	50″
"	929	3	2	275″	"	969	1	0	58″
"	930	5	1	668″	"	970	2	0	115″
Av. of five trials		**2.2**	**.6**	**235.6″**	**Av. of five trials**		**1.2**	**0**	**65.8″**
July 8	931	15	13	732″	July 18	971	9	2	257″
"	932	5	8	149″	"	972	0	0	72″
"	933	0	0	117″	"	973	0	0	56″
"	934	3	4	485″	"	974	0	0	57″
"	935	2	1	605″	"	975	0	0	46″
Av. of five trials		**5.**	**5.2**	**417.6″**	**Av. of five trials**		**1.8**	**.4**	**97.6″**
July 9	936	1	1	184″	July 19	976	1	0	241″
"	937	4	2	264″	"	977	0	0	43″
"	938	3	0	49″	"	978	5	2	275″
"	939	0	0	43″	"	979	1	1	94″
"	940	3	1	169″	"	980	4	2	216″
Av. of five trials		**2.2**	**.8**	**141.8″**	**Av. of five trials**		**2.2**	**1.**	**173.8″**
July 10	941	2	0	79″	July 22	981	7	0	217″
"	942	0	0	33″	"	982	0	0	25″
"	943	4	1	106″	"	983	0	0	32″
"	944	0	0	58″	"	984	0	0	42″
"	945	3	2	213″	"	985	0	0	68″
Av. of five trials		**1.8**	**.6**	**97.8″**	**Av. of five trials**		**1.4**	**0**	**76.8″**

Apparently the habit had degenerated. This may be attributed to (a) period of inactivity; (b) regeneration of ganglia and resumption of normal functioning bringing in new modes of reaction; or (c) a combination of these factors.

To the writer it seems probable that the interval of rest witnessed the birth of a new worm thru the regeneration of the nervous system. The habitual reaction which the "brainless" individual had exhibited now gave place to new modes of behavior initiated by the new "brain" cells.

Since it had been demonstrated that the previously acquired direction-habit had disappeared from the regenerated worm after an interval of four weeks' rest, training was instituted for the purpose of re-establishing the habit.

Gradually the behavior changed; the number of mistakes diminished and the trips became more direct. The data of table 8 indicate the nature and rapidity of the change which occurred between July 4 and July 22. Only one mistake was made in each of the series on July 16, 18, and 22.

It would seem therefore that worm No. 2, having (1) acquired a certain direction-habit as the result of systematic training, (2) lost the habit by reason of the regeneration of the ganglia of its anterior segments, and (3) exhibited a tendency to turn in the opposite direction to that demanded in the memory tests, which tendency (4) later was overcome by systematic training and gave place to a definite direction-habit.

SUMMARY

1. The manure worm *Allolobophora foetida,* is capable of acquiring certain modes of reaction which involve a definite direction of movement and the association of two stimuli.

2. The habit appears as a result of from 20 to 100 experiences. It is inconstant even when perfectly formed, varying markedly with the physiological condition of the worm ("good" and "bad" days) and with imperfectly controlled external conditions (temperature, moisture, light, etc.).

3. The results of training on the basis of five trials a day, or every other day, are more satisfactory than those obtained with 10, 15 or 20 trials a day.

4. There is a tendency to "track" (follow the slime or mucous path) if the apparatus is not thoroly cleansed between trials, but this tendency is not sufficiently strong or constant to yield perfect series.

5. Evidences of the effects of experience appear thruout the systematic training in (a) the increased readiness to enter the apparatus and to desert it for the exit tube; (b) the evident "recognition" of the exit tube; (c) the gradually increasing avoidance of the sandpaper, which was meant to serve as a warning against the electrical stimulus; (d) the acquired tendency to avoid contact with the electrodes; (e) the disappearance

of the tendency to attempt to retrace the course thru the stem of the T; (f) the similar disappearance of the tendency to turn back after progressing well toward the exit tube. These several bits of evidence, combined with the still more obvious increase in the number of correct or shortest trips, justify the conclusion that the worm is capable of profiting by experience in a simple maze.

6. The correct performance of a thoroly ingrained habitual act, of the kind studied in this investigation, is not dependent upon the "brain" (portions of the nervous system carried by the five anterior segments), since the worm reacts appropriately within a few hours after its removal.

7. As the brain regenerates, the worm exhibits increased initiative, its behavior becomes less automatic, more variable.

8. Within four weeks after the operation the regenerated segments appear superficially complete and the worm naturally burrows in a mixture of earth and manure.

9. Two months after the removal of the "brain," during the last four weeks of which period no training was given, the habit had completely disappeared from worm No. 2, the subject to whose responses this paper is devoted, and in its place there appeared a tendency to turn in the opposite direction to that demanded in the training.

10. Systematic training for two weeks resulted in the partial re-acquisition of the original direction-habit.

11. The various facts recorded in this investigation indicate that the removal and the regeneration of the first five segments resulted in the development of a worm strikingly different in behavior from the original worm, No. 2.

All of the statements of this paper are based upon the behavior of a single worm, and all of the conclusions are subject to modification in the light of results which are being obtained with other individuals. It has seemed to the writer desirable to present this individual study while the facts are fresh in his mind and the details of the behavior may be used to advantage.

The writer desires to acknowledge his indebtedness to his friend and colleague, Professor Herbert W. Rand, for valuable advice and aid in the investigation.

237

No. 15

MODIFIABILITY OF BEHAVIOR IN ITS RELATIONS TO THE AGE AND SEX OF THE DANCING MOUSE.

BY

ROBERT M. YERKES.

From the Harvard Psychological Laboratory.

WITH FOUR FIGURES.

CONTENTS.

I. INTRODUCTORY STATEMENTS: THE DANCER AS MATERIAL FOR THE INVESTIGATION OF PROBLEMS OF BEHAVIOR.

The dancing mouse is well adapted, by its abundant and in certain respects peculiar activity, to experiments on behavior. Taking advantage of this fact, I have used it extensively as material for the development of methods and the revelation of problems, both physiological and psychological. That the results which have been obtained are typically mammalian I am not prepared to assert. This, however, is, for my immediate purposes, secondary in importance to the methodological values of the work. Animal psychology is urgently in need of exact methods of research. It is an appreciation

of this fact that has shaped my experimental work during the past five years, and that now leads me to offer the following results of my study of the dancer primarily as a contribution to the evolution of method and as an aid to the profitable formulation of problems.

This paper is a direct continuation of the studies in the behavior of the dancer which are described in my book, "The Dancing Mouse."[1] Although I have attempted so to write the paper that both methods and results shall be intelligible to those readers who are not familiar with the details of previous publications,[2] it has been necessary—in order to keep my account within reasonable space limits—for me to omit everything except the chief points, in connection with methods which I have previously described, and a concise statement of new results. In other words, I have been forced to assume much more knowledge on the part of the reader than I should if this were my first publication on the subject.

Certain problems concerning the relation of age and sex to habit-formation which were proposed in my book, and either left unsolved or only partially solved, are brought nearer to satisfactory solution by the results herein reported, and a multitude of new problems are revealed. To me, however, the investigation presents itself simply as another step toward a realization of the complexity of the phenomena of behavior and of the need for accurate analytic methods.

II. RELATION OF AGE AND SEX TO RAPIDITY OF ACQUISITION OF A VISUAL DISCRIMINATION HABIT.

Can the dancer acquire a given habit with the same rapidity at different ages? This question was the starting point of a study of plasticity which has already been reported in part.[3] Before presenting the results of my experiments I shall very briefly, with the help of figures which are reproduced from an earlier paper, describe the method of work.

[1]Yerkes, Robert M. The Dancing Mouse: a study in animal behavior. New York, The Macmillan Company, 1907. xxi + 290.

[2]Yerkes, Robert M. and Dodson, John D. The Relation of Strength of Stimulus to Rapidity of Habit-formation. *Jour. of Comp. Neur. and Psy.*, vol. 18, p. 459-482, 1908.

[3]The Dancing Mouse, pp. 270-275.

The habit whose formation was studied quantitatively, in the case of groups of dancers consisting of five pairs each, for the ages of one month, four months, seven months, and ten months, may be called the white-black discrimination habit. It involved the discrimination

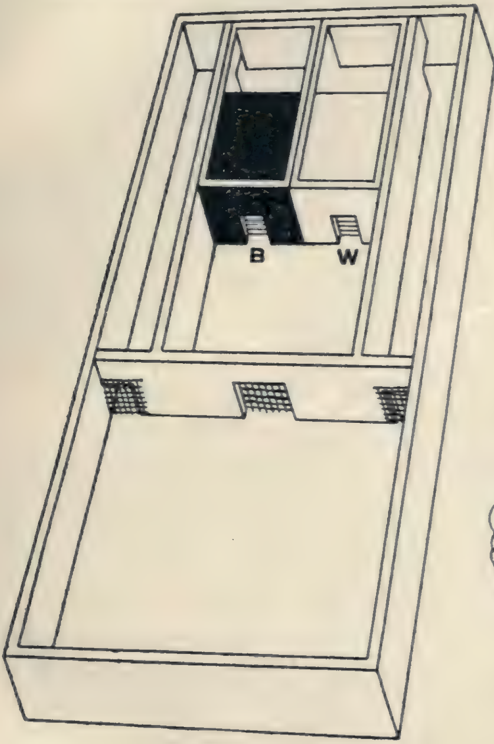


FIG. 1.

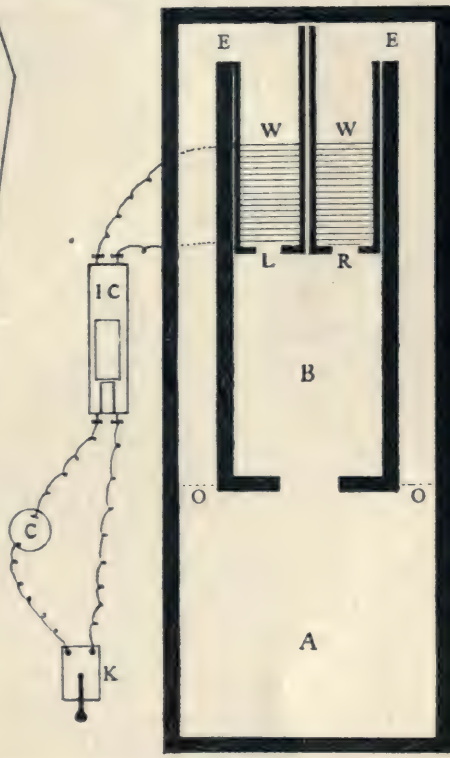


FIG. 2.

FIG. 1.—Discrimination box. *W*, electric box with white cardboards; *B*, electric box with black cardboards.

FIG. 2.—Ground plan of discrimination box. *A*, nest-box; *B*, entrance chamber; *W W*, electric boxes; *L*, doorway of left electric box; *R*, doorway from right electric box to alley; *O*, swinging door between alley and *A*; *IC*, induction apparatus; *C*, electric battery; *K*, key in circuit.

of the entrances to two boxes, one of which was white and the other black, and the entering of the white box. Any attempt to enter the black box was punished by an electric shock.

Figures 1 and 2 show the experiment box in perspective and in

ground plan, respectively. The subject, after being placed in the nest box, A, by the experimenter, was permitted to pass into the entrance chamber, B. Then a piece of cardboard, which was placed between the animal and the opening into A, was slowly moved toward L, R, of Figure 2. Thus the dancer was brought face to face with the two entrances, L and R, of this figure (B and W, *i. e.,* black and white of Figure 1). One of these it would soon attempt to enter in order to escape to the nest box, and thus find space for dancing. If it started to enter the black box (and this might be either the box on the left, L, or the box on the right, R, for the white and black cardboards, which were at the entrances and within the boxes, could be transferred readily by the experimenter) it was immediately given a weak electric shock by the closing of the key, K. This usually caused it to retreat from the box and to try the other entrance. In case it entered the black box in spite of the shock, it was not permitted to escape by way of E and O to the nest box, but instead was forced to return to B and again make choice of an entrance. This was continued until the white box was chosen, then the animal was allowed to return to A. After an interval of one or two minutes it was given another opportunity to select the right entrance. This was continued until the white box had been chosen ten times. Such a group of ten trials constitutes what we shall refer to as a series. One series was given each individual daily from the beginning of experimentation until the acquisition of a perfect habit of discrimination and choice.

The positions of the white and black cardboards were changed in precisely the same way for each individual according to an order which has already been described.[4] These shifts in the position of the white box were made in order to prevent the mouse from acquiring the habit of going regularly to the entrance at the left or at the right.

An experiment (test or trial) was recorded as yielding an error of choice if the mouse entered the wrong box far enough to get a shock; as yielding a correct choice if, without first entering the black box,

[4] *Jour. of Comp. Neur. and Psy.*, vol. 18, p. 461, 1908.

entered the white one and passed through to the nest box. In the tables appear the number of errors per series made day after day by the various individuals. At the outset of the experiments each mouse was given two series of what may be called "preference tests." In connection with these tests no electric shock was given and the mouse was permitted to enter and pass through either the white or the black box, for it was the sole purpose of the experimenter to discover, by means of these series, any initial preference that the subject might have for either the white or the black box.

A habit of discriminating between the boxes, and of uniformly choosing to enter the white one, was considered perfect when the mouse made no errors in three successive daily series. As a measure of the rapidity of habit-formation we may use the number of tests between the beginning of the first training series (following preference series B) and the end of the series which preceded the three perfect series. This measure of rapidity of learning, which I have named the index of plasticity, proves to be extremely useful for purposes of comparison.

To ascertain age differences in rapidity of white-black habit formation I used groups of individuals which, so far as I could tell, differed from one another constantly only in age. Five males and five females constituted each group, and four such groups were used. During their lives all of the animals were kept under the same conditions. They were paired at the age of twenty-five days, and thereafter a male and a female were kept in a separate cage and were placed in the experiment box for their daily training at the same time and given their tests alternately.

We may now examine the results of the experiments. Table 1 contains records of the number of errors of choice made by each of the individuals of the one-month-old group in each daily series. The numbers at the top of the columns refer to the mice. *Even numbers always designate males; odd numbers, females.* The two preference series are indicated by the letters A and B. No. 210, it will be noted, made six erroneous choices in each of the preference series and also in the first training series; that is, he attempted to enter the black box instead of the white box six times in ten. In subsequent training se-

TABLE 1.

RELATION OF AGE TO MODIFIABILITY OF BEHAVIOR
WHITE-BLACK DISCRIMINATION HABIT

Results for dancers one month old

	Males.						Females.					
Series.	210	250	252	254	410	Average.	215	249	251	253	415	Average.
A	6	6	2	7	6	5.4	8	5	6	4	8	6.2
B	6	3	5	6	5	5.0	8	6	5	5	6	6.0
1	6	7	5	5	2	5.0	7	6	6	3	6	5.6
2	4	1	4	4	2	3.0	5	3	4	2	2	3.2
3	3	3	2	5	3	3.2	3	4	2	3	3	3.0
4	5	3	4	1	3	3.2	2	1	4	3	3	2.6
5	3	3	5	1	1	2.6	1	1	4	2	3	2.2
6	2	4	2	1	0	1.8	2	3	2	0	1	1.6
7	1	1	1	2	1	1.2	1	1	1	1	3	1.4
8	0	1	0	1	0	0.4	0	1	1	1	2	1.0
9	0	1	0	0	1	0.4	1	0	2	2	1	1.2
10	0	1	0	0	0	0.2	0	1	0	0	0	0.2
11		0		0	0	0	0	0	1	0	1	0.4
12		0			0	0	0	0	0	0	2	0.4
13		0				0		0	1		0	0.2
14									0		0	0
15									0		0	0
16									0			0

TABLE 2.

RELATION OF AGE TO MODIFIABILITY OF BEHAVIOR
WHITE-BLACK DISCRIMINATION HABIT

Results for dancers four months old

	Males.						Females.					
Series.	76	78	114	122	126	Average.	75	77	111	115	117	Average.
A	7	7	3	5	6	5.6	4	8	8	6	5	6.2
B	8	6	4	6	8	6.4	6	5	4	7	5	5.4
1	5	5	5	7	6	5.6	5	5	4	6	6	5.2
2	5	4	5	7	5	5.2	2	2	4	4	2	2.8
3	4	5	4	5	5	4.6	2	5	3	3	3	3.2
4	3	4	5	3	3	3.6	1	1	5	4	2	2.6
5	5	2	4	3	3	3.4	0	1	3	5	2	2.2
6	3	2	5	1	1	2.4	1	0	1	5	2	1.8
7	2	1	4	4	1	2.4	1	2	3	2	3	2.2
8	5	1	2	4	2	2.8	0	0	1	1	1	0.6
9	1	3	2	2	2	2.0	0	0	5	1	0	1.2
10	1	2	1	1	0	1.0	1	0	1	0	1	0.6
11	1	1	3	1	2	1.6	0		1	2	0	0.6
12	1	1	0	1	0	0.6	0		1	0	0	0.2
13	0	0	1	0	1	0.4	0		1	1	0	0.4
14	0	0	0	0	0	0			0	0		0
15	0	0	1	0	0	0.2			0	0		0
16			0		0	0			0	0		0
17			0			0						
18			0			0						

ries the number of errors made by this individual rapidly decreased until in the seventh series only one was made. Then followed three perfect series. For this individual, since he acquired a perfect habit as the result of seventy training tests, the index of plasticity is 70.

The tables contain, in addition to the individual results, the average number of errors per series for the males and for the females.

TABLE 3.

RELATION OF AGE TO MODIFIABILITY OF BEHAVIOR
WHITE-BLACK DISCRIMINATION HABIT

Results for dancers seven months old

	Males.						Females.					
Series.	92	96	98	116	120	Average.	91	93	99	101	109	Average.
A	5	6	5	7	6	5.8	4	4	7	6	6	5.4
B	7	4	7	3	5	5.2	6	6	7	5	7	6.2
1	4	3	5	7	5	4.8	5	6	7	3	8	5.8
2	4	5	6	5	8	5.6	2	3	7	6	2	4.0
3	4	3	4	3	5	3.8	4	4	4	4	6	4.4
4	7	5	4	5	3	4.8	4	3	3	6	4	4.0
5	3	4	5	4	7	4.6	5	3	5	2	3	3.6
6	3	5	2	4	4	3.6	5	2	4	2	2	3.0
7	3	1	1	4	4	2.6	1	4	4	3	2	2.8
8	6	2	3	2	4	3.4	2	4	2	2	3	2.6
9	2	1	3	5	5	3.2	1	5	1	1	1	1.8
10	5	1	3	4	5	3.6	0	2	2	0	3	1.4
11	1	1	1	3	1	1.4	0	1	1	1	1	0.8
12	2	2	1	4	1	2.0	1	1	2	1	2	1.4
13	2	0	1	3	2	1.6	2	3	1	0	0	1.2
14	4	2	1	3	2	2.4	1	1	0	0	0	0.4
15	2	1	0	1	0	0.8	1	1	0	1	0	0.6
16	1	0	0	0	2	0.6	0	0	0	1		0.2
17	1	0	0	0	2	0.6	1	0		0		0.2
18	1	0		1	0	0.4	0	0		0		0
19	0			0	1	0.2	0			0		0
20	0			1	1	0.4	0					0
21	1			0	1	0.4						
22	0			0	1	0.2						
23	1			0	0	0.2						
24	0				1	0.2						
25	0				0	0						
26	0				0	0						
27					0	0						

Any one who compares this account of my investigation of the relation of age to rapidity of learning with my earlier account will discover that only two pairs of dancers of one month of age for which results were given previously[5] have place in the group under discussion. This is due to the fact that I felt it highly desirable to repeat the experiments with one-month individuals in order to make

[5] The Dancing Mouse, pp. 243, 273.

sure that in the interval which had elapsed between the beginning of this portion of my work and its completion no important changes in the plasticity of the race had occurred.[6] As a matter of fact this precaution proved unnecessary, for no important differences appeared as the result of the interruption of the investigation.

The condensed results for the four-month individuals appear simi-

TABLE 4.

Relation of Age to Modifiability of Behavior
White-black Discrimination Habit

Results for dancers ten and twelve months old

Males.							Females.					
Series.	90	112*	142	144	196	Average.	97*	113*	119	123	141	Average.
A	6	6	5	5	6	5.6	5	8	7	5	4	5.8
B	5	5	6	6	5	5.4	5	7	6	5	4	5.6
1	4	4	7	4	5	4.8	7	6	5	6	4	5.6
2	6	4	5	4	3	4.4	4	3	7	4	5	4.6
3	7	3	5	7	4	5.2	4	6	8	7	3	5.6
4	3	4	5	5	5	4.4	7	3	5	5	3	4.6
5	5	4	5	4	1	3.8	4	2	3	2	6	3.4
6	3	4	1	7	3	3.6	2	2	1	1	5	2.2
7	3	5	3	3	4	3.6	2	3	1	0	7	2.6
8	3	2	2	5	4	3.2	2	1	2	1	5	2.2
9	4	3	2	4	1	2.8	1	1	1	1	4	1.6
10	3	4	1	1	0	1.8	1	1	0	1	1	0.8
11	1	1	1	1	0	0.8	1	0	0	1	2	0.8
12	1	2	1	1	0	1.0	2	0	1	0	2	1.0
13	2	1	0	1		0.8	0	0	0	1	0	0.2
14	0	3	1	1		1.0	0		1	0	1	0.4
15	1	1	1	0		0.6	0		1	0	0	0.2
16	0	1	0	1		0.4			0	0	1	0.2
17	0	0	1	1		0.4			0		1	0.2
18	0	0	0	1		0.2			0		0	0
19		1	0	1		0.4					0	0
20		0	0	1		0.2					0	0
21		0		0		0						
22		0		0		0						
23				0		0						

* Twelve months old.

larly in Table 2, and those for the seven-month individuals in Table 3. In the ten-month group (Table 4) I have included the results for three mice which were twelve months old. Although it is not wholly satisfactory to do this, it seemed better than to deal with the three individuals separately. At any rate nothing is concealed by averaging the results for the ten mice, for the individual results are available.

To make comparisons easier, I have brought together in Table 5

[6] An epidemic which destroyed almost all of my mice caused a delay of over a year.

the averages for the males and females of each group. This table presents also the general averages for each sex. Inspection of these results reveals the following significant facts.

(1) The females exhibit a stronger initial preference for the black box than do the males. Both, however, choose the black box more frequently than the white box, in the preference series. Since

TABLE 5.

GENERAL RESULTS OF THE STUDY OF THE RELATION OF AGE TO MODIFIABILITY OF BEHAVIOR.

Each result in the table is either the average number of errors in ten tests for five individuals, or the general average for twenty individuals.

	MALES.					FEMALES.				
Series.	1 mo.	4 mo.	7 mo.	10 mo.	Gen. Av.	1 mo.	4 mo.	7 mo.	10 mo.	Gen. Av.
A	5.4	5.6	5.8	5.6	5.60	6.2	6.2	5.4	5.8	5.90
B	5.0	6.4	5.2	5.4	5.50	6.0	5.4	6.2	5.6	5.80
1	5.0	5.6	4.8	4.8	5.05	5.6	5.2	5.8	5.6	5.50
2	3.0	5.2	5.6	4.4	4.55	3.2	2.8	4.0	4.6	3.65
3	3.2	4.6	3.8	5.2	4.20	3.0	3.2	4.4	5.6	4.05
4	3.2	3.6	4.8	4.4	4.00	2.6	2.6	4.0	4.6	3.45
5	2.6	3.4	4.6	3.8	3.60	2.2	2.2	3.6	3.4	2.85
6	1.8	2.4	3.6	3.6	2.85	1.6	1.8	3.0	2.2	2.15
7	1.2	2.4	2.6	3.6	2.45	1.4	2.2	2.8	2.6	2.25
8	0.4	2.8	3.4	3.2	2.45	1.0	0.6	2.6	2.2	1.60
9	0.4	2.0	3.2	2.8	2.10	1.2	1.2	1.8	1.6	1.45
10	0.2	1.0	3.6	1.8	1.65	0.2	0.6	1.4	0.8	0.75
11	0	1.6	1.4	0.8	0.95	0.4	0.6	0.8	0.8	0.65
12	0	0.6	2.0	1.0	0.90	0.4	0.2	1.4	1.0	0.75
13	0	0.4	1.6	0.8	0.70	0.2	0.4	1.2	0.2	0.50
14		0	2.4	1.0	0.85	0	0	0.4	0.4	0.20
15		0.2	0.8	0.6	0.40	0	0	0.6	0.2	0.20
16		0	0.6	0.4	0.25	0	0	0.2	0.2	0.10
17		0	0.6	0.4	0.25			0.2	0.2	0.10
18		0	0.4	0.2	0.15			0	0	0
19			0.2	0.4	0.15			0	0	0
20			0.4	0.2	0.15			0	0	0
21			0.4	0	0.10					
22			0.2	0	0.05					
23			0.2	0	0.05					
24			0.2		0.05					
25			0		0					
26			0		0					
27			0		0					

entire lack of preference would be indicated by an equal distribution of the choices—five for the white and five for the black in each series—the preference for the black, in the case of the males, is .6 in series A and .5 in series B, and in the case of the females it is .9 in series A and .8 in series B.

(2) The females make more errors than the males in the first

training series, but thereafter they make fewer errors and their training is completed with fewer series than that of the males. In other words, the general averages of Table 5 indicate that a group of twenty female dancers, ranging in age from one month to twelve months, acquired the habit of discriminating between two boxes, whose only considerable difference was in amount of illumination, and of choosing the white box much more quickly than did a comparable group of twenty male dancers. This is especially interesting in view of the fact next to be noted.

(3) The one-month males exhibit a considerably less strong preference for the black box than do the one-month females and, at the same time, they acquire the habit much the more quickly. The reverse is true of the four-month groups: the males exhibit the

TABLE 6.

INDICES OF PLASTICITY FOR DANCERS OF DIFFERENT AGES.

Age of Individuals.	Males.		Females.		Both Sexes.	
	First correct series.	Total no. of training tests.	First correct series.	Total no. of training tests.	First correct series.	Total no. of training tests.
1 month	74	82	76	106	75	94
4 months	92	128	78	106	85	117
7 months	146	192	106	146	126	169
10 months*	128	160	98	134	113	147

* One male and two females whose ages were twelve months are included.

stronger preference for the black to begin with and learn somewhat less rapidly.

(4) The males acquire the white-black habit more quickly at the age of one month than at the ages of four, seven, or ten months. And the females likewise acquire the habit most readily at one month of age.

Several of the above facts are clearer in the light of the results of Table 6, in which are arranged the indices of plasticity for the several groups of dancers, and the number of trials which, in the case of each group, preceded the first correct series of choices. The indices are given in the columns headed "total number of training tests." Judging by these indices we may say that the plasticity of the male dancer, as measured by the particular habit under consideration,

diminishes from the age of one month to between the seventh and the tenth months. It seems then to increase slightly. Whereas 82 is the index for the one-month individuals, that for the four-month males is 128, and that for the seven-month males 192. For the ten-month group the index 160 indicates increasing instead of diminishing plasticity.

The results indicate that the plasticity of the females does not change greatly between the first and the fourth months; that thereafter it decreases for a few months, and then again increases slightly. The indices for the several ages, as they appear in Table 6, are 106, 106, 146, and 134. All of these except the first, indicate a degree of plasticity higher than that of the males.

Figures 3 and 4 represent graphically the principal results of the experiments which have just been described. Figure 3 is based upon the general averages of Table 5. The irregularly broken line is the curve of the learning for the males of the dancer race; the regularly broken line, for the females of the race. The superiority of the females in the acquisition of this particular white-black discrimination habit is apparent. Figure 4 is based upon the data of the third, fifth, and seventh columns of Table 6. The irregularly broken line may be termed the plasticity curve of the male dancer (for a particular habit) between the ages of one month and ten months. The regularly broken line may similarly be termed the plasticity curve of the female dancer between the same age limits. The solid line, the plasticity curve of the race.

And now we are confronted by the question, Why the age differences in plasticity which are exhibited by our results? In reply we might say that preference determines rapidity of learning. For we note that the females, apparently because of their strong preference for the black box, make more errors of choice in the first training series than do the males (general averages of Table 5), and that subsequently they very rapidly learn to avoid the black box. It would appear, then, that initial preference for the black box is a favorable condition for habit-formation because it leads to a large number of errors in the early training series and thus gives the animal that experience which enables it to adjust itself to the situation. This

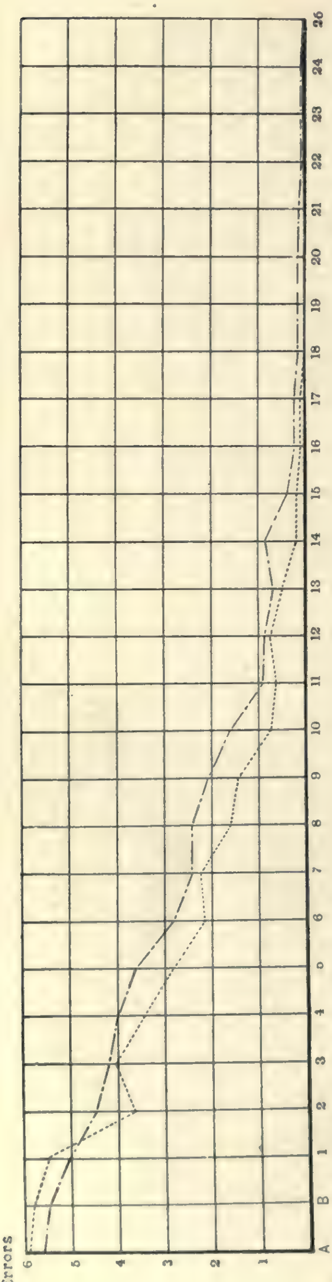


FIG. 3.

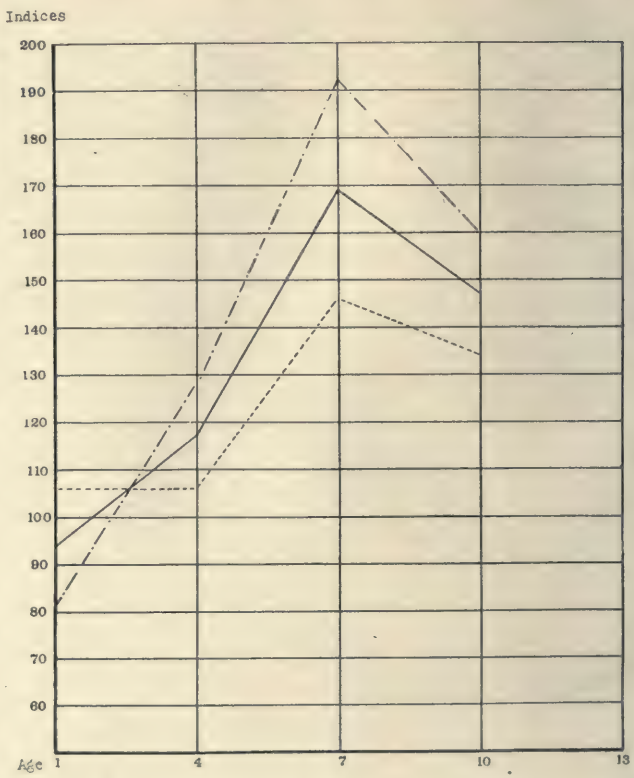


FIG. 4.

FIG. 3.—Curves of learning (error curves) for twenty male and twenty female dancers in white-black visual discrimination habit. Ordinates represent number of errors of choice in series of ten tests (per day). Abscissæ represent daily series of tests, beginning with two preference series, A and B, and continuing to the completion of the training. Curve for males — - — - — - ; Curve for females ....

FIG. 4.—Plasticity curves for twenty male, and twenty female dancers, and for the race, in white-black discrimination habit. Ordinates represent indices of plasticity or modifiability (*i. e.*, the number of tests up to the point at which no errors of choice were made in three successive series of ten tests each). Abscissæ represent ages in months. Curve for males — - — - ; Curve for females .... ; Curve for race ———.

interpretation of the facts, however, is contradicted by the following results which comparison of the data of Tables 6 and 7 reveals. The one-month males, although they showed only a slight initial preference (0.2) for the black box, acquired the white-black habit more quickly than did any other group of dancers. On the other hand, the four-month males, while exhibiting a strong initial preference (1.0) for the black box, acquired the habit much less quickly than did the one-month individuals. In view of these facts it is impossible to conclude that preference plays an all-important role as a condition for white-black habit-formation. Evidently we must look elsewhere for the factor or factors upon which the results of the plasticity experiments depend.

TABLE 7.

GENERAL RESULTS OF WHITE-BLACK PREFERENCE TESTS FOR INDIVIDUALS WHICH WERE USED IN THE STUDY OF THE RELATION OF AGE TO MODIFIABILITY OF BEHAVIOR.

The figures in the table represent the number of choices of the black in preference to the white in series of ten tests each.

Age of Individuals.	Males.			Females.		
	Series A.	Series B.	Average.	Series A.	Series B.	Average.
1 month	5.4	5.0	5.2	6.2	6.0	6.1
4 months	5.6	6.4	6.0	6.2	5.4	5.8
7 months	5.8	5.2	5.5	5.4	6.2	5.8
10 months*	5.6	5.4	5.5	5.8	5.6	5.7

* One male and two females whose ages were twelve months are included.

It is quite conceivable that age or sex differences in the value of the electrical stimulus may be responsible for the differences in rate of habit-formation which appear. This possibility was tested experimentally by an examination of (1) the relation of electric sensitiveness to age and sex, and (2) the relation of strength of stimulus to rapidity of habit-formation. Before attempting further to analyze or interpret the results presented in the tables we shall examine the experimental data which enable us to answer the questions, Does the sensitiveness of the dancer to electric stimuli depend upon age and sex, and, Does the strength of the electric stimulus influence the rapidity of habit-formation?

III. SENSITIVENESS TO ELECTRIC STIMULUS, IN ITS RELATIONS TO AGE AND SEX.

The measurements of sensitiveness to electrical stimulation which are now to be presented were made in connection with the study of the relation of age to rate of habit-formation, for the special purpose of throwing light upon the interpretation of the data which have just been considered. Had the experimenter's aim been to make a thorough-going investigation of the limits of sensitiveness in the dancer, other and more accurate methods would have been employed. But as matters stood, it seemed desirable to use for these tests of sensitiveness the method of applying the stimulus that had been used in the plasticity experiments themselves.

In its especially adapted form, this method exhibited the following points of importance. A current from a storage cell was used, in connection with a calibrated Hasler inductorium,[7] as stimulus. The strength of the induced current was regulated by moving the secondary coil. By means of an interrupted circuit device similar to that previously described[8] the mouse was permitted to receive this current through its fore feet. While one observer manipulated the keys of the circuits and regulated the strength of the current, another placed the mouse in position and observed its behavior when it received the shock. Determination was made, by repeated trials, of the lowest stimulus strength to which a definite motor response was given, and of the strength to which only an uncertain response was given. The average of these two results was accepted as *the threshold value for the individual.*

Twenty male and twenty female dancers were tested on two different days. The results in terms of the position of the secondary coil, as they appear in Table 8, indicate: (1) That the males are somewhat more sensitive than the females. This difference, which, according to the calibration curve of the inductorium, is nearly ten per cent., was not evident to the experimenter as he worked with the dancers from day to day in the training tests. (2) There is no indication of change of sensitiveness with increase in age.

[7]For the use of this inductorium I am indebted to Dr. E. G. Martin.

[8]The Dancing Mouse, p. 94.

As it happens the averages for the age groups are precisely the same for both the males and the females. This is a surprising result which we could not expect to obtain by the repetition of so small a number of tests. (3) Individual differences in sensitiveness are much more marked, and in all likelihood more important, than either sex or possible age differences.

As there is no reason to suppose, in the light of these rough determinations, that possible changes in sensitiveness which accompany ageing account for any of the results of the plasticity experiments, we may ask whether it is at all likely that the sex

TABLE 8.

MEASUREMENTS OF SENSITIVENESS TO ELECTRICAL STIMULUS.

SEX DIFFERENCES.

	Males (averages for 20).	Females (averages for 20).
Position of secondary coil, First Day	17.88+ cm.	17.41 cm.
Position, Second Day	17.72—	17.25
Average Position	17.80*	17.33
Extremes { Least sensitive	15.75	16.67
Extremes { Most sensitive	19.25	18.00

AGE DIFFERENCES.

	MALES.		FEMALES.	
	Not over four months.	Over four months.	Not over four months.	Over four months.
Number of dancers tested	13	7	12	8
Average age	2.5 mos.	8.3 mos.	2.4 mos.	8 mos.
Position of secondary coil, First Day	17.73 cm.	18.17 cm.	17.35 cm.	17.50 cm.
Position, Second Day	17.87	17.44	17.30	17.17
Average Position	17.80	17.80	17.33	17.33

* Difference in favor of males .47 cm., or about 10% of the value of the current.

and individual differences in sensitiveness furnish the basis for such differences in rapidity of habit-formation as the tables indicate.

Before this question can be answered satisfactorily, we must know what relation strength of electric stimulus bears to rapidity of learning. Does increase in the strength of the stimulus from the threshold value—or, what for our present purposes amounts to the same thing, increase in sensitiveness—facilitate or retard the process

of habit formation? As an answer to this question, I offer a summary statement of the results of a special investigation of the relation of strength of stimulus to rapidity of learning in which I was ably assisted by Mr. John D. Dodson. This study, unlike that of sensitiveness, was a thoroughgoing quantitative investigation of the significance of the factor under consideration, and we present our results with confidence that their accuracy, despite many technical difficulties, renders the generalizations which they indicate of importance not only in connection with our present experiments, but for all work on animal behavior.

IV. STRENGTH OF ELECTRIC STIMULUS, IN ITS RELATION TO RAPIDITY OF HABIT-FORMATION.

Precisely how does increasing or decreasing the strength of the electric stimulus, which the dancer is learning to avoid by associating it with the darker of two boxes, influence the process of learning? The answer which results obtained with forty dancers enable us to give to this question is exceedingly important in its several aspects.[9]

(1) We have demonstrated that the influence of the stimulus varies with the difficultness of the visual discrimination which is demanded of the mouse, and that condition of discrimination must be taken into consideration from the first in formulating our answer to the above question.

(2) That when visual discrimination is easy, rapidity of habit-formation increases as strength of stimulus is increased from the threshold to the point of injurious stimulation. In our experiments, the strongest stimulus employed was decidedly disagreeable to the experimenters and caused violent reactions in the mouse. Whether beyond this intensity of stimulation the rate of learning increases, we cannot say from the results of experimentation, but we may say with assurance that it cannot possibly increase very much, inasmuch as the stimulus would soon become positively harmful.

[9]Yerkes, R. M., and Dodson, J. D. The Relation of Strength of Stimulus to Rapidity of Habit-formation. *Jour. Comp. Neur. and Psy.*, vol. 18, pp. 459-482, 1908.

(3) That when visual discrimination is moderately difficult, rapidity of habit-formation increases as strength of stimulus is increased up to a certain point, and with further increase in the stimulus it rapidly decreases. A moderate strength of stimulus is most favorable for habit-formation under this condition of discrimination.

(4) That when visual discrimination is very difficult, rapidity of habit-formation increases as strength of stimulus is increased for a time, but not nearly so long as in the case of the medium condition of discrimination, and then with further increase in the stimulus it rapidly decreases. A low intensity of stimulus is most favorable for habit-formation under this condition of discrimination.

The law which is indicated by these facts may be formulated thus. *As difficultness of visual discrimination increases that strength of electrical stimulus which is most favorable to habit-formation approaches the threshold. The easier the habit the stronger that stimulus which most quickly forces its acquisition; the more difficult the habit the weaker that stimulus which most quickly forces its acquisition.*

From these facts it is evident that the value of a given strength of electric stimulus, for the training of a dancer whose sensitiveness is accurately known, can be stated only if the degree of difficultness of discrimination for the individual also be known. A degree of difference between the white and the black boxes which renders discrimination moderately easy for one dancer may render it extremely easy for another. Male and female, or old and young, or even two individuals of the same sex and age, may differ, both in discriminating ability and in sensitiveness.

This consideration makes apparent the incomparability of the results of the plasticity experiments. Instead of uniformity and simplicity of conditions, we have variability and complexity. It is evident that before a given individual can be used to advantage in any such training experiments as these, or rather before we can interpret the results, we must know accurately the relations of the conditions of experimentation to the individual.

Intensity of the electric stimulus is, then, important in connection with rapidity of habit-formation. Since, however, no difference in sensitiveness appears to be correlated with age differences we may assume, until we know otherwise, that the age differences in rapidity of learning are not due to the influence of the electric stimulus. But, at the same time, since the males appear to be more sensitive than the females, it may be that the sex differences in rapidity of learning are in part at least due to the influence of the stimulus. Possibly the particular combination of condition of discrimination and strength of stimulus was more favorable for the one-month males than for the comparable group of females, and possibly also for the females, as a whole, the combination of conditions was more favorable than for the males.

The significance of this suggestion will be clearer in the light of the results of the next section of this paper, for in that we shall have to examine data, which, if I could have foreseen them at the beginning of my work with the dancer, would have altered almost all of my experiments. I do not wish to give the reader the impression that I regard the results of the plasticity experiments as valueless or that I consider this investigation of mine exceptional in comparison with the work of any or all other investigators in this field. On the contrary, I have great respect for both the experimental procedure and the results which it yielded, but I am especially interested in pointing out the complexity of conditions which the investigation has revealed.

Before turning to the topic of the next section, I wish to call attention to the probable significance of the law of habit-formation which I have tentatively formulated above. As I have stated it, this law may not hold for other conditions of habit-formation, or for other animals. Only further investigation along lines which Mr. Dodson and I have followed can decide these questions. Meanwhile, it is evident that the subject is of great importance, for much of our experimental work in animal psychology rests upon the assumption that the stronger the stimulus which conditions a particular act the sooner the animal will learn to perform that act. In the light of our results concerning the relation of strength

of electric stimulus to rapidity of habit-formation it becomes pertinent to inquire, Is utter hunger as favorable a condition for the discovering of a certain method of obtaining food as moderate hunger? Is extreme eagerness to escape from confinement as favorable as a moderate desire? What we really should know before we undertake to study the intelligence of a particular animal is the value for it of the several factors which constitute the chief experimentally controlled conditions of activity. So long as we continue to use external conditions as incentives to habit-formation, without definite knowledge of their values for the individual, we shall work blindly. Food supply—the internal aspect of which is hunger—as a condition of habit-formation, may be studied experimentally; and the same is true of every other so-called motive upon which the experimenter depends. It is high time that we made serious efforts to discover the values of our stimuli instead of slothfully assuming that they will answer our purposes.

That there are a number of important laws of habit-formation to be discovered no student of animal behavior can doubt. These laws, of which the one offered above may serve as an example, should rapidly replace what is too much talked of as "the law of habit-formation."

V. RELATION OF DIFFICULTNESS OF DISCRIMINATION TO RAPIDITY OF HABIT-FORMATION AT DIFFERENT AGES.

Among the important results of the investigation of relation of strength of stimulus to rapidity of learning was the demonstration of the fact that differences in plasticity depend upon the condition of visual discrimination as well as upon the strength of the electric stimulus. What holds with respect to rapidity of acquisition of the white-black discrimination habit in young and old dancers, under conditions which render discrimination difficult, does not necessarily hold under conditions of easy discrimination. This I have demonstrated, and thrown further light upon, by three different methods, the results of which will now be presented in turn.

1. *Experiments with cardboards in discrimination box* furnished

the first indication of the great importance of condition of discrimination. With other points of method the same as in the plasticity experiments, I so arranged the black and white cardboards of the discrimination box that the amount by which the white box differed in illumination from the black box was very much greater than it had been in the earlier experiments. Whereas, formerly, discrimination had been rather difficult, it was now made

TABLE 9.

RELATION OF AGE TO RAPIDITY OF HABIT-FORMATION UNDER CONDITIONS OF DIFFICULT AND OF EASY DISCRIMINATION
WHITE-BLACK DISCRIMINATION

Series.	Dancers 8 or 12 Months Old.				Dancers 1 Month Old.			
	Difficult Discrimination.		Easy Discrimination.		Difficult Discrimination.		Easy Discrimination.	
	No. 112	No. 113	No. 204	No. 121	No. 292	No. 291	No. 430	No. 432
A	6	8	5	4	7	7	7	8
B	5	7	4	5	5	6	8	8
1	4	6	6	7	7	8	7	6
2	4	3	4	1	3	6	5	3
3	3	6	4	0	1	3	2	1
4	4	3	1	1	4	2	2	1
5	4	2	0	0	0	0	1	2
6	4	2	0	0	1	1	0	0
7	5	3	0	0	0	1	0	0
8	2	1	0*	0*	0	1	0	0
9	3	1	2	0	0	0	0*	0*
10	4	1	0	0		0	1	0
11	1	0	2			0	0	1
12	2	0	1				0	1
13	1	0	0				0	0
14	3		0					0
15	1		0					0
16	1							
17	0							
18	0							
19	0							

* At this point condition of discrimination was changed from "easy" to "difficult."

easy. The plasticity experiments, it is to be remembered, showed that the young dancers acquired the habit much more rapidly than the old individuals. Just the reverse proved to be true under the conditions of easy discrimination: the old mice learned more quickly than the young individuals.

In order to make the results perfectly conclusive, I carried out series of training experiments at the same time with a pair of

dancers one month old and a pair twelve months old under the conditions of discrimination used in the plasticity investigation, and similarly with two pairs of dancers one of which was one month old and the other eight months, under the conditions which I have just characterized as easy. The results of these experiments, as they are presented in condensed form in Table 9, are striking indeed. As was the case in my first experiments, the old dancers[10] acquired their habit, under conditions of difficult discrimination, much less rapidly than did the young individuals. The index of plasticity for the twelve-month mice, Nos. 112 and 113 of the table, is 130; that for the one-month mice, Nos. 292 and 291, is 70. The latter acquired the habit with few more than half as many training tests as were necessary for the former.

When we turn to the results of the experiments made under conditions of easy discrimination, we find that the eight-month mice, Nos. 204 and 121, learned with only 40 tests; whereas, the one-month individuals, Numbers 430 and 432, required 50 tests.

In Table 10 are presented the results of additional experiments like those just described. Two eight-month dancers, Nos. 136 and 166, acquired the habit on the basis of 40 and 20 tests, respectively. Their index of plasticity is, therefore, 30. The index for two four-month mice, which were subjected to the same training, was 80, and that for two eight-month individuals, 75.

The importance of the relation of age to difficultness of visual discrimination is clearly exhibited by the indices of plasticity for young and old dancers under conditions of easy and difficult discrimination in Table 11.

Comparison of the data of Tables 9, 10 and 11 with those of Tables 1 to 6 proves conclusively that the direction of the age differences in plasticity which was revealed by the experiments described early in this paper was determined, in part at least, by the condition of visual discrimination which happened to be chosen for the

[10]I shall use the terms of old and young in contrasting two groups of dancers which differed in age by several months. As a matter of fact a dancer at the age of eight, ten, or even twelve months is not, as a rule, obviously senile.

experiments. Had the tests been made with a condition of greater difference in the illumination of the two boxes the results probably would have indicated a slight increase in plasticity with age, instead of a decrease. If then under one condition of training plasticity diminishes as the dancer grows older, and under another condition in connection with the same habit it increases, it is clear that the

TABLE 10.

RELATION OF AGE TO RAPIDITY OF HABIT-FORMATION UNDER CONDITIONS OF EASY AND OF DIFFICULT DISCRIMINATION. WHITE-BLACK DISCRIMINATION.

Discrimination at first easy and later difficult.

Series.	Dancers 8 Months Old.		Dancers 4 Months Old.		Dancers 1 Month Old.	
	No. 136	No. 166	No. 408	No. 185	No. 416	No. 105
A	5	6	4	4	8	8
B	7	4	4	3	6	3
1	6	4	6	5	7	3
2	5	1	5	3	4	4
3	0	0	3	3	5	6
4	1	0	3	1	4	3
5	0	0	2	3	7	2
6	0	2*	1	2	2	1
7	0	2	0	1	0	1
8	1*	4	0	0	0	1
9	1	3	1	0	0	1
10	0	2	0	0	3*	0
11	2	2	0	0*	4	0
12	0	3	0	1	6	0
13	0	1	1*	0	2	1*
14	0	0	1	0	3	0
15		0	0	0	1	0
16		2	1		1	0
17		0	0		2	
18		0	0		0	
19		1	0		1	
20		1			0	
21		1			0	
22		2			0	
23		0				
24		0				
25		0				

*At this point condition of discrimination was changed from "easy" to "difficult."

relation of age to rapidity of habit-formation is more complex than certain statements made by students of animal behavior would lead one to suppose.

My experiments reveal the presence and importance of a number of variable factors in the white-black discrimination habit; and until we know accurately the values and relations of these several

factors it would be rash indeed to make general statements concerning the relation of age to plasticity. We must limit ourselves carefully to particular statements, for what holds of one condition of training may not hold at all of what appears to be a very similar condition.

TABLE 11.

INDICES OF PLASTICITY FOR DANCERS OF DIFFERENT AGES, TRAINED UNDER CONDITIONS OF DIFFICULT OR OF EASY VISUAL DISCRIMINATION.

No. of dancer.	Condition of Discrimination.			
	DIFFICULT.		EASY.	
	Young dancers–1 month old.	Old dancers–12 months old	Young dancers–1 month old.	Old dancers–8 months old.
112		160		
113		100		
204				40
121				40
292	60			
291	80			
430			50	
432			50	
136				40
166				20
416			60	
105			90	
Averages.	70	130	62.5	35.0

2. *Experiments with discrimination box in dark-room.* The results of the experiments which have just been described suggested to me the idea that ability to acquire the white-black visual discrimination habit depends largely upon two factors: capacity for visual discrimination and associative memory. The facts of plasticity thus far revealed might be accounted for, it would seem, by the assumption that in the young dancer capacity for visual discrimination was either greater at the outset or more readily developed than in the case of old individuals, whereas associative memory is more highly developed in the old than in the young mice. This hypothesis I immediately attempted to test experimentally. If it be correct, young mice should develop the capacity to discriminate slight differences in luminosity more quickly than old mice. To test this matter I planned a series of training experiments with the apparatus which I have previously described[11] as the Weber's

[11]The Dancing Mouse, p. 118.

law apparatus. It is a discrimination box in which the two boxes which have heretofore been referred to as white and black are illuminated by standardized incandescent lamps. There are no cardboards and difference in illumination, as desired, is obtained by shifting the position of the source of light for one of the boxes. This apparatus permits easy and fairly accurate measurements of the absolute and relative illumination of the two boxes, and in this respect it is more satisfactory than the cardboard method. Its chief disadvantage is that it compels experimentation in a dark-room or at least with artificial illumination of the boxes.

In the Weber's law apparatus two pairs of dancers were trained systematically until they had been given almost a thousand tests. The individuals represent the age limits of the plasticity experiments. The old ones, Nos. 170 and 95, were ten and twelve months, respectively; the young ones, Nos. 294 and 293, were one month old. Instead of a single series of ten tests per day, all these individuals were given two such series each day.

To start with, all the mice possessed perfectly formed habits of choosing the white box, in the old white-black discrimination apparatus. Experiments in the Weber's law apparatus were begun with the two boxes illuminated the one by 80 hefners, the other by 20 hefners. The difference in luminosity in this case may be stated as three-fourths, since the latter value is only one-fourth the former. I have found it convenient to keep one of these values constant throughout a training experiment and to vary the latter as need dictated. The fixed value, which may then be known as the *standard,* is indicated in the table by the abbreviation S. The other value, which may be known as the *variable,* is indicated by the abbreviation V.

A habit was considered perfect in this experiment when a dancer succeeded in choosing without error in two successive series. As soon as ability to discriminate a certain degree of difference in luminosity had been acquired, the amount of the difference was reduced and the training continued under the more difficult condition of discrimination. We may now examine the results of this experiment as they appear in Table 12.

At the outset the condition of discrimination was fairly easy and the old dancers learned to choose correctly with 110 tests, the young ones, with 95. In view of the results of the previous section we might have expected the young individuals to learn more slowly than

TABLE 12.

RELATION OF DISCRIMINATING ABILITY TO AGE.
EXPERIMENTS WITH WEBER'S LAW APPARATUS.

Dancers 10–12 Mo.			Dancers 1 Mo. Old.		
Series	No. 170	No. 95	Series	No. 294	No. 293
S. 80 h.V. 20 h. Difference three-fourths.					
1	6	8	1	9	6
2	4	5	2	8	5
3	7	6	3	3	4
4	5	3	4	6	4
5	4	3	5	4	1
6	1	3	6	5	0
7	1	3	7	1	0
8	1	2	8	5	0
9	1	1	9	3	1
10	0	0	10	3	0
11	0	2	11	1	0
12	2	0	12	2	0
13	0	1	13	1	..
14	0	0	14	1	..
15	0	0	15	0	0
16		0	16	0	0
S. 80 h. V. 40 h. Difference one-half.					
17	3	1	17	0	2
18	2	0	18	1	2
19	3	3	19	5	4
20	2	1	20	1	1
21	0	3	21	1	3
22	1	3	22	1	1
23	3	0	23	3	2
24	1	1	24	2	5
25	1	2	25	2	5
26	1	2	26	0	3
27	1	1	27	4	3
28	0	0	28	5	0
29	2	1	29	2	0
30	1	1	30	0	2
31	1	0	31	1	0
32	0	1	32	0	0
33	2	2	33	1	2
34	1	0	34	4	0
35	2	2	35	2	0
36	2	1	36	0	0
37	2	2	37	1	..
38	3	1	38	1	..
39	1	0	39	0	..
40	0	2	40	0	..
41	1	1			
42	0	0			
43	0	0			
S. 80 h.V. 60 h. Difference one-fourth.					
44	3	3	41	4	3
45	1	5	42	2	4
46	4	3	43	4	3
47	3	4	44	3	7
48	6	3	45	8	6
49	3	4	46	3	7
50	1	2	47	2	6
51	4	4	48	3	4
52	4	5	49	2	5
53	3	4	50	3	6
54	3	2	51	2	3
55	3	3	52	2	3
56	2	2	53	4	4
57	1	4	54	3	4
58	3	5	55	4	3
59	2	5	56	4	2
60	2	6	57	4	5
61	4	5	58	3	5
62	2	4	59	2	3
63	2	4	60	3	5
64	3	6	61	6	7
65	2	5	62	3	5
66	5	6	63	3	4
67	3	4	64	3	7
68	2	3	65	2	3
69	2	4	66	1	2
70	2	4	67	0	4
71	3	4	66	4	3
72	3	5	69	2	3
73	3	2	70	4	0
74	2	4	71	5	2
75	2	3	72	2	3
76	1	2	73	3	4
77	1	2	74	2	5
S. 80 h. V. 26.66 L. Difference one-third.					
78	2	4	75	2	0
79	1	3	76	2	2
80	1	5	77	2	3
81	2	6	78	1	2
82	1	1	79	0	1
83	4	5	80	0	1
84	2	5	81		3
85	1	3	82		0
86	1	2	83		4
87	4	2	84		2
88	2	3	85		3
89	3	5	86		3
S. 80 h.V. 32 h. Difference two-fifths.					
90	1	1	87		1
91	1	3	88		3

the old ones. But we must remember that the conditions of this experiment are markedly different from those in which cardboards were used to render the two boxes visually distinguishable.

Next the amount of difference in luminosity was reduced to one-half, and the experiment continued. Again the young individuals acquired the habit more quickly than the old ones. The index of plasticity for the old is 250, for the young it is 165.

With a difference in luminosity of only one-fourth, the training was now continued for several days, but as no one of the four mice succeeded in acquiring a perfect habit it was changed finally to one-third. It is noteworthy that in the thirty-four series (340 tests) that were given to the mice with the difference one-fourth, the old individuals did not succeed in making a correct series, whereas both of the young mice did. With the difference one-third, No. 294 quickly acquired a perfect habit, and No. 293 came very near to doing so, but failed in twelve series. At the conclusion of the twelfth series, neither of the old individuals had learned to choose correctly, with the difference one-third.

Although the results of this experiment are not as convincing as they might be, they do indicate that young dancers can acquire the ability to discriminate slight differences in luminosity more readily than can old individuals. It is conceivable, then, although by no means demonstrated as true, that the young individuals in the plasticity experiments acquired the white-black habit more quickly than the old individuals did because they could discriminate better or acquired discriminating ability more rapidly and not because they acquired an association more readily. In this event, our experiment measures differences in visual discrimination, and in changes which it undergoes with training instead of associative plasticity.

I have already shown[12] that the dancer is capable, as the result of prolonged training, of developing the power to discriminate between boxes which differ from one another in illumination by less than one-tenth. This fact becomes important at this point, for

[12] The Dancing Mouse, pp. 127, 128.

we are forced to ask, Do the plasticity experiments reveal anything except age differences with respect to what might be termed the educability of light vision? With the hope of getting further light on this problem, I carried out additional experiments, with the individuals used in the Weber's law apparatus, by a method whose form and results will now be described.

3. *Experiments with one side of discrimination box covered in varying degrees.* For this work the cardboards were removed from the discrimination box which had served for the plasticity experiments, and difference in the illumination of the two boxes was obtained by covering, with a piece of black cardboard, the whole or a part of the top of one of the two small boxes. The total inside length of the boxes was 29 cm. I have described the condition of the darker box by giving in terms of a fraction the amount of the top which was covered. Thus 18/29 means that the cardboard covered 18 of the 29 cm., beginning at the entrance and extending toward the rear of the box. Shifting the lighter box (the one to be chosen) from side to side involved merely the moving of the black cardboard from the top of one box to the top of the other.

After the experiments just reported had been completed, mice Nos. 170, 95, 294, and 293 were given training tests in the discrimination box under the above conditions. Table 13 presents the condition of discrimination as well as the results of the various series of tests. When, as at the outset, the whole of one box was covered, discrimination was extremely easy, because the boxes differed greatly in illumination.

From the first, as the data of Table 13 indicate, the young animals learned more rapidly than did the old ones. We have in these results, therefore, additional support for the belief that discriminating ability is more readily gained by the young dancer.

It may not be out of place to remark here that the simple form of the lighter-darker discrimination apparatus which served for this series of experiments is precisely what should have been used throughout this investigation. It has taken me years to learn that it is not only possible, but also perfectly easy, to devise a condition of experimentation which should be readily and accurately

describable as to the difference of brightness of the two boxes and satisfactory in its results. I cannot too strongly urge, from my present point of view, the avoidance of cardboards as means of testing visual discrimination. The conditions of many of my experiments are practically indescribable so far as absolute value of illumination is concerned, yet, as I now see it, they might perfectly well have been describable with a fair degree of accuracy.

TABLE 13.

RELATION OF AGE TO ABILITY TO DISCRIMINATE ON THE BASIS OF DIFFERENCE IN ILLUMINATION, AND TO THE CAPACITY FOR IMPROVEMENT OF VISUAL DISCRIMINATION.

Series.	Dancers 10–12 Months Old.			Dancers 1 Month Old.		
	Portion of darker box covered by card.	No. 170.	No. 95.	Portion of darker box covered by card.	No. 294.	No. 293.
1	Whole.	5	5	Whole.	5	3
2		2	6		4	4
3		2	1		0	1
4		1	0		0	0
5		0	0		0	0
6		0	0	$\frac{18}{29}$	0	0
7		0	0	$\frac{15}{29}$	1	1
8	$\frac{18}{29}$	2	1	$\frac{12}{29}$	0	0
9	$\frac{15}{29}$	3	0	$\frac{9}{29}$	0	0
10	$\frac{15}{29}$	3	0	$\frac{6}{29}$	5	2
11	$\frac{12}{29}$	0	1	$\frac{6}{29}$	3	1
12	$\frac{12}{29}$	2	2	$\frac{6}{29}$	4	2
13	$\frac{9}{29}$	2	0	$\frac{6}{29}$	2	1
14	$\frac{9}{29}$	5	0	$\frac{6}{29}$	4	2
15	$\frac{6}{29}$	5	2	$\frac{6}{29}$	2	1
16	$\frac{6}{29}$	2	2	$\frac{6}{29}$	1	3
17	$\frac{6}{29}$	4	1	$\frac{6}{29}$	0	1
18	$\frac{6}{29}$	4	1	$\frac{6}{29}$	2	2
19	$\frac{6}{29}$	4	2	$\frac{6}{29}$	1	0
20	$\frac{6}{29}$	4	1			
21	$\frac{6}{29}$	2	3			

Having now tested the first of the two important factors in the the acquisition of the white-black discrimination habit, namely, ability to gain visual discriminating power (the educability of white-light vision), we must turn to the second factor and inquire whether associative memory changes with age.

It occurred to me that since the labyrinth habit, as conclusively proved by Professor Watson[13] for the white rat, depends more

[13]Watson, J. B. Kinæsthetic and Organic Sensations: Their Role in the Reactions of the White Rat to the Maze. *Psychol. Rev. Mon. Supp.*, vol. 8, no. 2, 1907. vi + 100 pp.

largely upon kinæsthetic sense data than upon vision or any other special sense, it might serve well to reveal age differences in associative ability. In this connection we may ask, therefore, Do old dancers learn labyrinth paths more readily than young ones?

VI. RELATION OF AGE TO RAPIDITY OF ACQUISITION OF LABYRINTH HABITS.

For the labyrinth experiments I selected two mazes which I had previously used for the study of educability in the dancer: they are designated as D and C in my book.[14] D is what I have described as the regular type of maze, and C as the irregular. Training in labyrinth D was given first to each of ten dancers of from one to two months of age, and likewise to the same number of about ten months of age. About a month after the completion of the training in labyrinth D, the same individuals were trained in labyrinth C.

In all cases the experiments were conducted as follows. Two mice, a male and a female from the same cage, were placed in the nest box of the labyrinth together. One at a time they were given first a preliminary test in which they were permitted to find their way from the entrance to the exit of the labyrinth without being disturbed, and then training tests in which they received a slight electric shock each time they made an error in the choice of a path. The tests were continued without interruption, first one individual then the other being tested, until each had perfectly learned the path. A habit was considered as perfect when an individual succeeded in traversing the maze twice in succession without a mistake. Records were kept of the number of errors in choice of a path and of the time consumed in finding the way from entrance to exit.

As typical series of results I present in Table 14 the time and error records in labyrinth D of No. 416, a six-week dancer, and No. 166, a nine-month individual. The young mouse was slower than the old one in most of the tests, but he acquired a perfect habit

[14]The Dancing Mouse, pp. 219, 222.

no less quickly. I need scarcely state that the time records have little value for our present purposes, and are therefore omitted, except in the case of Table 14.

TABLE 14.

RELATION OF AGE TO RAPIDITY OF ACQUISITION OF LABYRINTH HABITS. TYPICAL SERIES OF RESULTS GIVEN BY TWO MALES IN LABYRINTH D.

Number of trial.	Dancer 6 Weeks Old. No. 416.		Dancer 9 Months Old. No. 166.	
	Time in seconds.	Number of errors.	Time in seconds.	Number of errors.
Preliminary.	280	31	161	19
1	240	60	56	6
2	134	24	25	4
3	53	6	62	8
4	22	0	143	17
5	141	3	62	8
6	15	0	57	6
7	22	1	31	3
8	14	0	15	0
9	63	6	46	3
10	58	2	29	2
11	15	0	20	0
12	12	0	13	0

TABLE 15.

RELATION OF AGE TO RAPIDITY OF ACQUISITION OF LABYRINTH HABITS.

Results in the table indicate the number of trials up to the point at which no errors occurred for at least two consecutive trials.

Dancers 1–2 Months Old.			Dancers 10 Months Old.		
Number of animal.	Results for Labyrinth D.	Results for Labyrinth C.	Number of animal.	Results for Labyrinth D.	Results for Labyrinth C.
256	5	27	92	15	22
258	8	22	96	6	6
396	15	26	98	6	19
398	16	14	120	9	12
418	13	7	166	10	—
Av. for Males.	11.4	19.2	Av. for Males.	9.2	14.7+
179	4	16	91	19	8
181	19	9	93	15	13
255	10	18	97	6	11
263	6	7	99	8	19
395	13	—	109	10	7
Av. for Females.	10.4	12.5	Av. for Females.	11.6	11.6
Gen. Av.	10.9	16.2	Gen. Av.	10.4	13.0

The general results of the labyrinth experiments appear in Table 15. Averages are given for the sexes separately, inasmuch as so

often heretofore we have discovered sex differences to be of importance for the interpretation of our results. Comparing the young dancers with the old, we note that the males of one to two months of age acquire both the labyrinth D and the labyrinth C habits considerably less quickly, as measured by the number of tests, than the ten-month individuals. In the case of the two groups of females there is practically no difference in rapidity of learning. The general averages likewise show that the old dancers are somewhat superior to the young in ability to learn these labyrinth paths.

What evidence we have, favors the conclusion that the associative memory of the dancer improves somewhat during the first year of life. Possibly change in the "apperceptive mass" is responsible. It is only fair to admit, in concluding the presentation of experimental data, that I consider this problem unsolved, for I have presented insufficient evidence to convince the critical observer that associative memory improves with age.

VII. CONCLUSIONS AND SUMMARY.

This attempt to discover the relation of age and sex of the dancing mouse to plasticity, or rapidity of habit formation, makes possible interesting and important, albeit not altogether favorable, comments upon the methods of the investigation. As the work progressed it became increasingly clear that the use of cardboards as means of producing different degrees of illumination of the two boxes between which the mouse was forced to discriminate was unsatisfactory. Chief among the objections to this method may be mentioned the practical impossibility of keeping the difference in illumination constant throughout even a single series; the impossibility of determining accurately, except by very elaborate and time-consuming methods, either the relative or the absolute illumination of the white and black boxes;[15] the impossibility of chang-

[15]Photometric determinations of the amount of light reflected by the white and the black cardboards which were used throughout these experiments indicate that the white cardboard reflected about 10.5 times as much light as the black cardboard. For a careful measurement of these values of the cardboards I am indebted to Professor J. W. Baird.

ing the amount of difference in the illumination of the boxes with ease and accuracy. These are only a few of the objections to white, grey, and black cardboards or papers that experience enables me to raise. Naturally I shall neither use them nor recommend their use hereafter in investigations of the visual powers of animals. Later, in connection with a report on "Methods of studying vision in animals" which is to be made by the committee on standardization of tests of the American Psychological Association, I shall propose a substitute method.

The investigation has shown, I believe, the great importance of choosing conditions of experimentation which may be readily and accurately measured and controlled, and of determining, as a preliminary to any experimental study of habit-formation, the value for the individual animal of the several important factors in the experimental situation. It has further shown that we should work with *individuals,* and with relatively simple and perfectly analyzable situations; and that no treatment of our results is so likely to hide their real significance as the averaging of groups of observations for different individuals. Evidently the best preparation for an experiment is a thoroughgoing study of the characteristics of each animal to be used in the investigation; and the best result which an experiment can yield is evidence of the relation of experimentally controlled conditions to the particular traits of an individual animal. Averages are important, but we should not sacrifice individual facts for the purpose of presenting them.

The primary aim of the investigation, it will be remembered, was the discovery of the relation of plasticity to age and sex. The data prove that dancers at the age of one month acquire the white-black habit more rapidly than do older individuals, and that the females, on the average, acquire the habit more rapidly than do the males. Of great importance is the fact (represented by the curves of Fig. 3) that whereas the females make more mistakes of choice at first than do the males they very soon begin to choose with a higher degree of accuracy, and ultimately acquire a perfect habit with considerably fewer training tests than the males.

That these age and sex differences in the form of the habit-

formation curves are not necessarily indicative of general differences in plasticity is rendered evident by the results of the sections on sensitiveness, strength of stimulus, and difficultness of discrimination. For in the light of these results we are able to name as two important, and to a certain extent independently variable, conditions upon which the acquisition of the visual habit depends, (a) ability to sense the difference in illumination of the two boxes and (b) ability to associate the darker box with the electric shock. Evidently an individual which possesses highly developed white light vision and is capable of distinguishing very slight differences in illumination may, at the same time, possess little ability to associate stimuli. The data of section V indicate that what we have called age and sex differences in plasticity are in all probability, to be referred to differences in visual discriminating ability and in "associative memory." Admitting that the results of the experiments justify only tentative conclusions, we may say that the young dancer seems to be somewhat superior to the old individual in ability to discriminate on the basis of difference in illumination, whereas the old individual seems to associate stimuli somewhat more rapidly, if anything, than the young dancer.

This suggestion, for it is scarcely more than that, of the way in which a habit may break up into two relatively independent factors is one of the most interesting results of the investigation. It strongly emphasizes the importance of studying the various sense factors separately and of attempting to discover upon what external and internal conditions "associative memory" depends.

To sum up the results of the investigation point by point, it appears that—

1. The dancer at one month of age acquires a particular white-black visual discrimination habit more rapidly than do older individuals. From the first until the seventh month there is a steady and marked decrease in rapidity of habit-formation; from the seventh to the tenth month the direction of change is reversed. These statements hold for both sexes.

2. Young males acquire the habit more quickly than young females, but between the ages of four and ten months (at least) the females acquire the habit the more quickly.

3. Curves of learning for the sexes indicate that the female makes more mistakes early in the training tests than does the male, but that this condition soon gives place to greater accuracy of choice on the part of the female.

4. Initial preference for the white or the black box does not seem to be a very important determinant of the rate of habit-formation.

5. Tests of sensitiveness indicate that the male dancer is somewhat more sensitive to electric stimuli than the female. There are no evidences of changes in sensitiveness with change in age.

6. The strength of the electric stimulus which is used as an incentive for habit-formation is extremely important as a determinant of rate of habit-formation. For a given animal and condition of visual discrimination there is a certain strength of stimulus which is most favorable for the acquisition of the habit (the optimal stimulus).

7. It is extremely important that experimenters discover the optimal stimulus for habit-formation.

8. For the dancer the following law appears to hold in connection with the particular habit under consideration and for the electric stimulus. *As difficultness of visual discrimination increases that strength of electric stimulus which is most favorable (the optimal) to habit-formation approaches the threshold.* The easier the habit the stronger that stimulus which most quickly forces its acquisition; the more difficult the habit the weaker the stimulus which most quickly forces its acquisition.

9. A given difference in the illumination of the boxes which are to be discriminated cannot be detected with equal ease by old and young dancers. When the difference in illumination is slight the young individuals detect it more readily than the old mice; when the difference is great, the old individuals apparently detect it as readily as do the young mice.

10. The capacity for "associative memory" is greater, if anything, in the dancer of ten months of age than in the one-month individual. This is indicated by certain results of the visual discrimination experiments and by the results of labyrinth tests.

11. The results of this investigation indicate, then, that the acquisition of a visual discrimination habit depends upon two independently variable (within limits) capacities in the dancer: (1) the power to detect differences in illumination or to gain this power (educability of white-light vision), and (2) the power to associate the darker box with the electric shock (associative memory). The former of these capacities seems to be greater in the young than in the old dancer; the latter seems to be somewhat greater in the old than in the young individual.

12. Should the statements just made hold true for animals generally, it is evidently important that the senses be trained early in life and that the development of associative memory be furthered later. Investigation of the problems suggested by our results should yield important practical data for the science of education.

[Reprinted from the PSYCHOLOGICAL BULLETIN, Vol. VII, No. 8, Aug. 15, 1910.]

No. 16

DO KITTENS INSTINCTIVELY KILL MICE?[1]

BY ROBERT M. YERKES AND DANIEL BLOOMFIELD,
Harvard University.

The observations which we here report supplement in certain respects the results obtained by Berry[2] in this laboratory, and suggest the need of alterations in his conclusions concerning the reactions of kittens to mice. Upon his study of the behavior of a litter of Manx kittens Berry based the following statements, which we wish the reader to consider in connection with the facts which it is the purpose of this paper to present. "I am also led to believe that cats are credited with more instincts than they really possess. It is commonly reported that they have an instinctive liking for mice, and that mice have an instinctive fear of cats. It is supposed that the odor of a mouse will arouse a cat and that the odor of a cat will frighten a mouse. My experiments tend to show that this belief is not in harmony with the facts. When cats over five months old were taken into the room where mice were kept they did not show the least sign of excitement. A cat would even allow a mouse to perch upon its back, without attempting to injure it. Nor did the mice show any fear of the cats. I have seen a mouse smell of the nose of a cat without showing any sign of fear."

"It was not until the mouse began to run that the interest of the cat was aroused. The cat then ran after it, playfully striking it with her paw, becoming rougher the longer she played with it. The instinct seems to be for the cat to run after anything which runs from it. I think it is evident that it is through imitation that the average cat learns to kill and eat mice. If this is true, it shows the extreme importance of imitation in the mental development of the cat. Further-

[1] From the Psychological Laboratory of Harvard University.

[2] Berry, C. S., 'An Experimental Study of Imitation in Cats,' *Jour. of Comp. Neurol. and Psychol.*, Vol. 18, pp. 1–25, 1908.

more, it indicates that much that has commonly been attributed to instinct is, in reality, due to imitation" (pp. 24–25).

Believing that the experiments upon which the above generalizations are based should be repeated with younger kittens than those observed by Berry, we have systematically studied the reactions of eight kittens when in the presence of mice.

The kittens belonged to two litters. Of these, the first, consisting of four kittens, was born in the laboratory on March 2, 1910, of a black and white cat which had been obtained through an animal dealer in Boston a few days previously. The second litter, also of four kittens, was born on May 11, 1910, of a maltese and white cat which had been furnished by the same dealer two weeks previously. Hereafter we shall refer to the several kittens by number as follows:

First Litter Born March 2, 1910.		Second Litter Born May 11, 1910.	
No. 1	Male	No. 5	Female
No. 2	Male	No. 6	Male
No. 3	Female	No. 7	Male
No. 4	Female	No. 8	Male

The cats, and later the kittens also, were kept in the animal-room of the laboratory, with the freedom of the room much of the time. No other animals were kept in the room, and special precautions were taken to make the room mouse-proof, so that the observers might obtain complete histories of the kittens' experiences with mice. The diet of the cats consisted of fresh milk, beef (usually cooked), and fish. The kittens were given fresh milk as soon as they would accept it, which was during the fourth week. All of the individuals were in perfect health throughout the experiments and exhibited normal development, but the members of the first litter were better nourished, more active, energetic, and aggressive than those of the second.

In reporting our results we shall describe first the experiments with the eight kittens previous to the opening of their eyes.

Each individual was tested, in the presence of both observers, at least twice during the first week of life, with the odor of mouse. This was done by bringing a live mouse near to the nose of the kitten. In no instance was a reaction observed which differed essentially from the reactions to such odors as weak ammonia, sour yeast, leather, and the experimenter's hand. The animals gave no evidence of special interest in the odor. Usually there occurred twitching of the nose; rarely opening of the mouth; and still more rarely, something between hissing and spitting.

From these observations, which we report thus briefly because of limited space, we conclude that during the period of post-natal blindness kittens tend to avoid the unfamiliar odor of mouse, just as they avoid other strange odors, and, further, that the odor has no special significance with which a definite instinctive reaction is correlated.

In stating the results of our tests with the animals after they had gained their sight, we shall deal with the litters separately.

These tests consisted in placing each kitten separately in a cage with wire mesh sides and cover, 45 cm. by 30 cm. by an average of 20 cm. deep, in which a mouse had been placed. Here the kitten was kept under continuous observation for fifteen minutes. The eyes of the members of the first litter, nos. 1–4, were fully opened on March 14, and on that date the animals were tested, at the age of twelve days. They exhibited no reactions to the mouse, beyond looking at it momentarily as it happened to approach or touch them. Neither the olfactory nor the visual stimuli from the mouse called forth anything which resembled an instinctive reaction. In general it may be said that the kittens paid no other attention to the mouse than to a bit of dry bread in the cage.

On April 1, when slightly over four weeks old, nos. 1–4 were again tested. The first three reacted to the mouse only by following it now and then with their eyes as it happened within the field of vision. They neither chased nor made attempts to touch it. The behavior of no. 4 contrasted markedly with that of the others. She noticed the mouse, soon after she had been placed in the cage, as it moved near her, and quickly seized it in her mouth, growling the while. The mouse escaped and the kitten gave chase, but failed to recapture it before it had climbed to the top of the cage. In this position the kitten seemed unable to recognize it, and she made no further attempts to find it during the fifteen minute interval.

In the behavior of no. 4, a female, at the age of four weeks, we thus have unmistakable evidence of the presence of the killing instinct. Had the mouse been smaller, or had the cage rendered it more easily accessible, it certainly would have been killed. It was evident that the kitten's stage of visual development did not enable it to follow the rapidly moving mouse or to recognize it at a distance of twenty to thirty centimeters.

Five days later, on April 6, the kittens of the first litter were tested for the third time.

No. 1 approached the mouse and smelled of it, but he drew back quickly when his nose touched it. He seemed to be startled, mewed

loudly, and tried to escape from the cage. Later he several times approached the mouse and smelled of it as at first. There was no evidence of a killing instinct. We have repeatedly seen the kittens react similarly to inanimate objects.

No. 2 smelled of the mouse, when the latter's movements had attracted its attention. It also followed it about the cage with its eyes for brief periods. Interest soon failed and the kitten mewed persistently to be let out of the cage.

No. 3 at first tried to escape from the cage. Upon seeing the mouse move she trembled noticeably, as if in fear. Later, when the mouse happened to touch her, the kitten spit and hissed vigorously.

No. 4 smelled about the cage as though searching for something. She finally saw the mouse in one corner of the cage and made for it, but she was too slow to catch it, for the mouse had quickly darted to the top of the cage as the kitten began to move. The kitten continued to the place where she had seen the mouse an instant before, and after feeling and smelling in the corner where the mouse had been she licked her paw. Soon she located the mouse at the top of the cage. She approached, reached up with one paw and touched the mouse, then drew back. This was repeated three times. Had the mouse run from its position, the kitten doubtless would have attempted to catch it. After a few minutes the kitten again noticed the mouse, this time nibbling at a bit of dry bread on the floor of the cage. She immediately approached it, and as it ran she pursued and tried to catch it. As the time for the test had elapsed the kitten was now removed to the animal-room.

On April 8, the fourth test was given kittens nos. 1–4.

No. 1 showed more interest in the mouse, by following it readily and persistently with the eyes and moving about after it. When he happened to touch it, he spit and struck at it with his paw playfully (?). The mouse was not afraid of the kitten and did not run away.[1]

No. 2 attended immediately to the mouse and began to play with it. After a time he accidentally sat down on the mouse, whereupon it squeaked. This aroused the kitten and caused it to strike with its paw and lick its lips. It then followed the mouse about the cage at intervals.

[1] These experiments have revealed marked differences in the behavior of mice toward kittens. We have used wild gray mice, and laboratory gray, black, and agouti mice. Under the conditions of our experiments, the wild mice always exhibited extreme fear, whereas of the laboratory mice some were obviously fearful and others fearless of the kittens. Whether the differences are due to experience or to inheritance we have not attempted to determine.

No. 3 behaved as in the previous test. When the mouse came near her, she would spit and move away.

No. 4 exhibited characteristically interesting behavior. On being placed in the cage, with back arched she pursued the fleeing mouse to a corner and then spit and struck at it with her paws. Next she bent her head low and sniffed at it. She watched it closely for a time, showing sustained attention. When the mouse finally attempted to escape from the corner, the kitten made a dash for it, hissing and growling. On overtaking her prey, she drew back slightly, spitting and repeatedly unsheathing and sheathing the claws of a paw which she extended toward the mouse. The mouse next made a sudden dash across the cage. Like a flash the kitten was upon it mouth and claws. With the mouse held firmly by the head, the kitten began to growl. In a few seconds she shifted her hold and taking the nose of the mouse she bit it repeatedly. Then followed a period of worrying during which the mouse several times tried to escape only to be seized by some part of its body and vigorously chewed or struck with the paws of its captor. Within fifteen minutes of its capture the mouse had been bitten and worried to death. The kitten immediately began to chew at one of its legs, which it finally succeeded in eating, but the skin of the mouse was so tough that the kitten soon abandoned its attempt to eat its prey.

At this point, in order to determine the effect of delaying for several weeks the development of the ability of a kitten to kill mice, tests with nos. 1 and 3 were discontinued and only nos. 2 and 4 were used.

On April 14 the latter individuals were given their fifth test. No. 2 smelled of the mouse and drew back. He followed it momentarily with his eyes, but soon lost interest and paid no further attention to it. No. 4 chased the mouse, as in the previous test, spitting, striking with exposed claws and otherwise exhibiting excitement. Owing, however, to the ability of the mouse to keep out of reach most of the time, the kitten did not succeed in capturing it within fifteen minutes.

April 20 was the date of the sixth test. No. 2 watched, followed, and struck at the mouse repeatedly. He showed more sustained attention than previously. No. 4 behaved as in the foregoing test. She did not succeed in capturing the mouse.

On April 22 the tests were made in the mouse-room of the laboratory instead of in the experiment room which had been used for the other tests. No. 2 exhibited no new reactions. He did not follow the mouse persistently. No. 4 seized the mouse immediately by the head, growling as she chewed at it. She had eaten it completely within six minutes.

Again on April 25 no. 2 was tested in the mouse-room. From the first he followed the mouse and struck at it with his paw. Soon he succeeded in cornering it and seizing it by the neck and back. Growling he chewed vigorously at its head and back until it was dead. Within five minutes he had eaten his prey. In this test the kitten's interest in the mouse increased greatly when the latter showed fight.

Finally, on April 27 both no. 2 and no. 4 were again tested in the mouse-room. No. 2 at first attended to the mouse playfully, but suddenly as the mouse ran across the floor of the cage this playfulness was replaced by the killing instinct. The kitten gave chase, seized the mouse, and by biting it through the head killed it immediately. When another kitten was brought near the cage no. 2 growled warningly.

No. 4 immediately began to sniff about the cage. As soon as she caught sight of the mouse she ran and seized it. The mouse managed to escape, whereupon the kitten excitedly gave chase and again caught it. This time she bit its head so severely that it was helpless. Later the mouse was killed and eaten.

On May 16 a different sort of test was given. Each kitten was set free in the mouse-room with a mouse. No. 2 was interested in the mouse, but he showed only slight ability to keep track of it among the furnishings of the room (cages, table, radiator, boxes, etc.). Nevertheless, he had discovered it, by reason of its movements, within a few minutes, and he immediately captured it. Subsequently, after playing with the mouse for several minutes — repeatedly allowing it to escape and then recapturing it, tossing it into the air, clawing, and striking it with its paws, etc.— the kitten killed its prey by biting it about the head, and ate it. No. 4 also had difficulty in keeping track of the mouse. She spent much time smelling about the room as if in search of it. Finally she saw the mouse move. Capture followed. The captive was then played with for a while, after which it was killed by being bitten through the head and neck, and eaten.

This concludes our brief description of the behavior of nos. 2 and 4. We have shown that these two kittens — the female within a month after birth and the male in less than two months — without opportunity for imitation and without other experience with mice than that described above, began to kill and eat mice. We feel fully justified, in view of the characteristics of the behavior, in calling it instinctive.

With the thought that the instinctive tendency to kill mice might wane or become increasingly difficult to develop as kittens aged, we postponed tests with nos. 1 and 3 until May 26. These kittens were

therefore almost three months old when we resumed our tests with them.[1]

On May 26 nos. 1 and 3 were given their fifth test, the fourth having been given on April 8. This test was given in the mouse-room. No. 1 appeared to be aroused by the odor of mouse in the room and ran about when brought into the room, as if searching for something. This observation, which we made in connection with several of the kittens, would seem to indicate that the odor of mouse may become significant to the kitten even before it has made its first kill. When placed in the experiment box with a large gray mouse, the kitten smelled of it several times, followed it about the cage, and finally struck at it with his paw, immediately thereafter springing back as the mouse moved. Nothing further of interest happened during the interval of experimentation.

No. 3 smelled about, located the mouse, and watched it intently. She hissed as it moved. Subsequently she followed the mouse closely and struck at it with claws exposed. Once she brushed it from the top of the cage with a paw. As it ran to one corner of the cage, she followed and struck it several times with her paw: her tail switched and her ears twitched a great deal during this activity. During the remainder of the interval, she clawed at the tail of the mouse as it cowered in a corner of the cage.

Again on May 28 the kittens were tested in the mouse-room. No. 1 was attentive to the mouse from the first. He watched and followed it, growling the while. It was evident that the movement of the mouse attracted him. At last the mouse attempted to run across the cage. The kitten made a sudden dash for it, seized it by the back, and at once killed it by biting through the spine in several places. He then switched his tail, growled, and hissed as he stood over his prey. Repeatedly he picked up the body, sometimes tossing it into the air, and each time striking it with his paw as it fell to the floor. The mouse was not eaten.

No. 3 licked her lips when placed in the cage, and her ears twitched noticeably as she crouched and watched the mouse. Occasionally she spit. As the mouse ran to the top of the cage she followed and struck it. With back arched she watched it intently and steadily for a few seconds, then she brushed it down with a paw. This happened a number of times. At the end of the interval the kitten was closely watching and following the mouse.

[1] It is to be remembered that the kittens studied by Berry were at least five months old when he first tested them with mice.

The eighth test was given to nos. 1 and 3 on June 1. No. 1 caught the mouse as soon as he saw it. He growled as he bit through the back of the mouse and killed it. About half of the body was eaten at once. No. 3 behaved as in the previous test. She did not attempt to kill the mouse.

On June 3 only no. 3 was tested. She smelled about the cage, and when she discovered the mouse at the top she crouched. She then stealthily approached and struck with exposed claws. As the mouse retreated she followed. In less than a minute she had seized it and so severely bitten its neck that it was helpless. The kitten growled, hissed, and otherwise exhibited excitement the while.

The results of these tests with two kittens three months old, when considered in connection with those of Berry on kittens five months old, suggest that the killing instinct (of the existence of which we are convinced by our observations) tends to become increasingly difficult to evoke as the animals age. This in part, doubtless, accounts for the fact that some cats will not kill mice at all, whereas other are more or less eager to do so.

We are inclined to believe that Berry's results would have led him to quite another conclusion (he definitely states "that cats do not instinctively kill and eat mice, but learn to do so by imitation," p. 25) had he tested his Manx kittens at the age of one to two months in a cage which did not permit the mice readily to get out of sight or reach of the kittens.

The behavior of the members of the second litter, Nos. 5–8, will be described only with respect to certain points in which it differed from that of the kittens of the first litter.

As previously stated, the reactions of Nos. 5–8 while yet blind furnished us with no indications of an instinctive adjustment to the odor of mouse. After the opening of their eyes, these individuals were systematically tested on May 26 and 29, and on June 1 and 3. In none of these tests did anything noteworthy which has not already been recorded appear.

Beginning with the test of June 7, when the kittens were about four weeks old, we may describe the behavior of the several individuals. For this test small gray mice about ten days old were used.[1] Their eyes were not yet open and they of course could not run from the kittens. They crawled about the cage considerably during the experiments.

No. 5 sniffed when placed in the cage, in the mouse-room. Her attention was taken by the movements of the mouse and she looked at

[1] In all previous tests well grown mice had been used.

it for several seconds. Although she did not touch it with her paws at first, she several times placed her nose close to it, and finally licked it gently with her tongue. Next she took it in her mouth and bit it hard enough to make the mouse squeak, but not sufficiently to break the skin. This was repeated three times. Then the kitten left the mouse. She gave no signs of excitement or of an instinct to kill. Rather her behavior was indicative of mild interest and playfulness.

No. 6 looked at the mouse momentarily as it crawled about and touched it with his nose, but he paid no further attention to it.

No. 7 was attracted by the movements of the mouse and touched it with his nose. He then left it. After twelve minutes he happened to be so placed that he could see the mouse as it began to move. For a few seconds he watched as if fascinated by the sight. Then he moved directly and quickly, in spite of shakiness of his legs, to the mouse, and seized it in his mouth by the middle of the back, at the same time biting hard, and bending his head to the floor so that one paw could be placed firmly on the body of the mouse. In a few seconds, he had bitten the mouse to death. Without pause the process of eating was begun. It proved a difficult task, notwithstanding the tenderness of the mouse, but after ten minutes of diligent effort and much gagging it was completed.

Here we have a kitten one month old, still so feeble and unpracticed that he tottered as he attempted to walk, seizing, placing his paw upon, and eating a small mouse after the manner of cats. Anything more striking in the way of an exhibition of the killing instinct would be difficult to imagine. The observation proves that given conditions which favor its appearance—namely, a mouse suited in size and strength to the size of the kitten and a cage which does not permit it to get beyond the kitten's field of vision — the killing instinct may appear even during the first month of life. Although meat had been fed to the mother, it is extremely improbable that this kitten had ever before tasted meat of any sort, and it is certain that he never before had tasted mouse flesh. Indeed, he had only a few days before the test begun to lap milk from the food dish.

No. 8 merely looked at the crawling object for a second or two and then lay down for a nap.

On June 16 all of the kittens were again tested. No new reactions appeared. Not one of the individuals except No. 7 made any attempt to follow or capture a mouse. He caught, killed, and ate his mouse in a calm business-like way.

The members of the second litter were further tested on June 26,

27 and 28. On none of these dates did any of them kill a mouse. On the twenty-seventh no. 5 struck and bit one so badly that it lay as if dead, but she paid no further attention to it after once seizing and then dropping it.

After the twenty-eighth of June the experiments had to be discontinued, notwithstanding the fact that the records for the members of the second litter were incomplete.

Rather dogmatically stated, and with meager discussion, our conclusions are as follows:

1. Kittens possess the instinct to kill mice. We are not prepared to say that the reaction to mice differs essentially from that given to other small living things, but we have clearly demonstrated by a variety of tests that the reaction to a mouse differs radically from that given to lifeless objects which are moved before the animal.

2. The instinct to kill may manifest itself in the kitten before the end of the first month of life, while the animal is yet feeble and barely able to eat a young mouse. It more commonly appears during the second month.

3. The instinct appears suddenly. In a moment the playful kitten becomes transformed into a beast of prey. The picture of the play instinct differs as greatly from that of the killing instinct as does the picture of joy from that of rage.

4. The reaction is fairly definite in character, complex, and highly adaptive. It involves the bodily states of attention; muscular tension; bristling of the hair; sometimes erection and sometimes switching of the tail; hissing and, at times, spitting; growling; unsheathing and sheathing of the claws; use of the mouth, teeth, paws, and claws. The picture varies somewhat with individuals.

5. The instinctive reaction is aroused (*a*) by the movement of the mouse (if the animal keeps quiet it is not noticed by the inexperienced kitten); (*b*) by the odor of the mouse (this seems to be relatively unimportant for the first reaction; subsequently it is of great importance). Berry is quite right in saying that the kitten has an instinct to chase any small object that runs from it. If the object when overtaken behaves as does a mouse, the killing reaction is likely to appear. We should say, then, that the visual factor of movement is the primary condition for the initiation of the killing instinct; that the odor of a mouse becomes markedly significant with the first kill and subsequently plays a part; and that the visual impression of the form of a mouse becomes important as the visual ability of the kitten develops. It is almost certain that cats usually recognize mice by their odor and movements and not by their form or markings.

6. The instinct does not completely wane during the first three to five months of a kitten's life, but it apparently becomes increasingly difficult to evoke. The practical inference is: allow a kitten to exercise its killing instinct when young if a good mouser is desired.

7. Although opportunity neither for imitation nor for experience with mice is necessary for the efficient execution of the killing reaction by kittens, there can be no doubt that each of these conditions ordinarily contributes to the awakening of the killing instinct. Cats bring dead or injured mice, or other small animals, to their kittens. Thus, early in life, the animals become familiar with the odors of their natural prey. Undoubtedly, then, the average kitten would react to a mouse during the second month of life more quickly than did those observed in the laboratory.

The whole point of our work, it is to be noted, is the study of the instinct to kill as it is exhibited by kittens which have been deprived of everything in the nature of preparation for dealing with mice.

8. We deem as chiefly important in our observations, the fact that kittens, even in their first kill, so seize the mouse that they cannot be bitten by it. In almost every instance our kittens caught their mice by the head, neck, or back in such a way that the animals were helpless. We present this fact as a point in favor of the specificity of the reaction to mice. An untutored observer certainly would have inferred from the behavior of these kittens that they had learned just how to seize mice in order to prevent them from biting.

9. The killing instinct with surprising rapidity lends itself as the basis for the acquisition of habits of dealing with mice. Racial and individual experience soon become so completely intermingled in the behavior of a kitten toward mice that only in the light of a minute and thorough knowledge of the latter can the instinctive features of the behavior be identified. Whereas at first the kitten tends to kill immediately upon capturing a mouse, it thereafter tends rather to delay the fatal bite. At first the reaction is performed in a business-like way; later the kitten plays with its prey for minutes at a time without seriously injuring it. Again, the beginner attempts to capture a mouse only when it runs, whereas the experienced kitten begins to search for its prey as soon as it sees the box in which it once killed a mouse, or as soon as it detects the odor of mouse.

10. It is our impression, although statistically our results do not justify a statement of fact, that in the female kitten the instinct to kill is more highly developed than in the male.

BEHAVIOR MONOGRAPHS

Volume 1, Number 2, 1911 Serial Number 2

Edited by JOHN B. WATSON
The Johns Hopkins University

Methods of Studying Vision in Animals

ROBERT M. YERKES
Harvard University
AND
JOHN B. WATSON
The Johns Hopkins University

THIRTEEN FIGURES

A report prepared for the Committee of the American Psychological Association on the Standardizing of Procedure in Experimental Tests

Published
at Cambridge, Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E. STECHERT & CO., London, Paris and Leipzig, Foreign Agents

INTRODUCTION

In 1907 we were asked by a Committee[1] on the standardization of psychological methods, appointed by the American Psychological Association, to attempt to devise "a standard procedure for testing color vision in animals." This we undertook to do. Four years have been devoted to the task, and during that time the scope of the investigation has been extended to include methods of studying the perception of (a) light (colorless), (b) size, (c) form, and (d) distance, as well as (e) color. We thus broadened our task because knowledge of the light vision of an animal is necessary for the intelligent study of its color vision, and, further, because it seemed to us highly desirable that closely related methods should be employed in the quantitative investigation of the several aspects of vision in an organism.

We beg leave now to submit to the Committee a full report of our researches. Although we feel encouraged by the progress which we have made in our search for highly controllable methods of studying the several aspects of vision, we realize that the methods which we recommend as standard procedures should be accepted rather as improvements upon procedures now in common use than as new and perfect methods. Indeed, we would urge the desirability and the fundamental importance of continuous efforts to improve upon our present methods

It is with pleasure that we express our indebtedness and gratitude to the following individuals and scientific firms for varied assistance.

Especially in connection with the spectral color apparatus, several physicists have aided us greatly. In fact, the construction of the apparatus would hardly have been possible without the aid of Professors Millikan and Gale of the University of Chicago and Professors R. W. Wood, A. H. Pfund, and J. A. Anderson of the Johns Hopkins University. For mechanical assistance we thank Mr. C. M. Childs, mechanician attached to the Physical and Psychological Laboratories of the Johns Hopkins

[1] Consisting of Professors Angell, Judd, Pillsbury, Woodworth, and Seashore.

University, and Mr. William Gaertner of Chicago. Mr. B. Spencer Greenfield, formerly mechanician in the Harvard Psychological Laboratory, has aided us in many ways, but especially by making drawings of the various parts of our apparatus.

For information and advice on varied topics we thank Professors G. H. Parker, H. W. Morse and G. W. Pierce of Harvard University; Doctor Louis Bell, consulting electrical engineer, Boston; Professor J. W. Baird of Clark University; Professors Edward L. Nichols and E. B. Titchener of Cornell University; Mr. A. J. Marshall and Mr. G. A. Anderegg of New York; Mr. Willard Greene; Doctors P. G. Nutting and W. W. Coblentz of the United States Bureau of Standards; Mr. James Wallace of the Cramer Dry Plate Company; Eimer and Amend; Metz and Company; the National Electric Lamp Association; Mr. J. G. Biddle, and the Arthur H. Thomas Company.

The late Professor W. A. Nagel of the University of Rostock, master of the technique of physiological optics and throughout his life a specialist in the problems of vision, was especially painstaking in his efforts to further our investigation. His death in the prime of life leaves with us a keen sense of personal loss. We desire to express our appreciation of the man and the scientist.[1]

During the early portion of our investigation, we profited by the valuable assistance of Doctor E. G. Congdon. He worked especially on the problems of ray filters. The authors wish to emphasize the importance of Doctor Congdon's share in the investigation. Except for the fact that he is not in any way responsible for the methods and apparatus which we have during the past two years decided to perfect and recommend as standard procedures, his name should appear as a joint author of this monograph.

Professor Yerkes, who was charged by the Committee with the general direction of the work, is chiefly responsible for the preliminary portion of the report and for the method and apparatus

[1] After this manuscript had been written, with the exception of the final descriptions of the light and color apparatus, there appeared in Tigerstedt's *Handbuch der physiologischen Methodik* an admirable review and discussion of "*Methoden zur Erforschung des Licht- und Farbensinns*" by Professor Nagel. Since Professor Nagel in his paper presented some of the material which we had intended to publish, it has seemed to us desirable to omit certain portions of our report, as originally planned. We especially commend Professor Nagel's discussion to those who are interested in human vision.

recommended for the study of "*light vision.*" To Professor Watson chiefly belongs the credit for the method and apparatus herein described and recommended for the study of "*color vision.*"

We have attempted, in this report, first, to indicate pointedly the principal merits and defects of several of the more important and commonly employed methods of studying vision, and, second, to perfect and recommend, *for all intensive quantitative investigations*, a procedure for testing light, size, form, and distance perception, and a procedure for testing the various aspects of color perception. It is our hope that this monograph may prove of service to other investigators by enabling them to avoid the use of eminently unsatisfactory, although commonly used, methods, and by facilitating the adaptation to their special needs of what we judge to be the more promising types of procedure.

At the outset, it is essential that we emphasize the ground upon which we recommend certain procedures for standardization. We have neither discovered nor devised perfect methods; we have merely done our best to propose methods whose scientific value is obvious and whose possibilities of improvement seem almost unlimited. Our standard procedures are recommended only for thoroughgoing, intensive, quantitative work. Simpler and more conveniently manipulated apparatus may be used in the case of preliminary exploratory work. We do not wish to discourage the use of crude and relatively uncontrollable methods for the study of vision, but we do most emphatically recommend that these methods be abandoned as soon as the rough problem-defining portion of an investigation has been completed. The employment of colored papers, colored yarns, painted backgrounds, and similar conditions, for the testing of color vision may yield valuable qualitative results, and they may be used to advantage if the experimenter realizes and clearly states that his control of the visual conditions of his tests is extremely imperfect. In this methodological investigation, it has been our aim to perfect procedures which shall vastly increase the experimental control and describability of the visual stimuli to which an organism is subjected.

Without further introduction, we shall present our results in accordance with the plan given in the table of contents.

I. METHODS OF INVESTIGATING *LIGHT* VISION

1. Sources of stimulus

A source of stimuli for the study of *light* vision [1] in animals should fulfill four important requirements which may be designated as naturalness, controllability, constancy, and measurability.

(1) Naturalness.—The source should yield light which is identical with, or closely similar to, daylight or sunlight in quality. Or, in other words, stimuli which are used for the investigation of the visual capacity of an animal should be "familiar" or "natural" to the animal. As has been pointed out by Nichols, "the normal stimulus of the eye is diffuse daylight and artificial stimuli which depart widely from it either as to intensity or quality are sure to be unsatisfactory and are likely to be injurious." [2] We may expect natural or normal reactions only when stimuli to which the animal is accustomed are employed. This is the principal reason for the use of daylight and sunlight, instead of artificial light, in experiments on vision.

(2) Controllability.—The source of light should be, so far as is possible, under the control of the experimenter. This requirement applies to both quality and intensity, but it is especially important in connection with work on *light* vision that the intensity of the stimulus be accurately controllable.

(3) Constancy.—The source under given conditions should yield light whose quality and intensity are constant. No known source fulfills this requirement. Certain of the high temperature electric lamps are most nearly satisfactory.

(4) Measurability.—The light which a given source produces should be readily and accurately measurable in terms of visual (e. g., photometric) and energy (e. g., radiometric) units. *In the employment of visual, and all other stimuli, it is desirable to obtain three sets of measurements: the psychological* (*sensation-unit*), *the physiological* (*stimulus-unit*), *and the physical* (*energy-unit*).

With respect to their relations to the four requirements which lights used for experiments on vision should fulfill we shall

[1]Frequently termed "brightness" vision.

[2]Nichols, E. L. Daylight and artificial light. Illuminating Engineer, 1908.

now examine the most important and widely available natural and artificial sources of light.

a. The sun.—For the eyes of most animals, it is well known that daylight and sunlight are the natural photic stimuli. Indeed, it is in connection with the reception of such stimuli that the eye has developed. As pertinent evidence of the truth of this statement we may mention the fact that the maximum of the luminosity curve for the human eye and that of the energy curve of sunlight lie in the same region of the spectrum.

The sun fulfills neither the second nor the third of our four requirements: it is uncontrollable experimentally and inconstant. To a degree which few untrained observers realize, the composition or color values of daylight and sunlight vary with the condition of the atmosphere, the altitude of the observer, the hour, and the season. Like statements may be made concerning intensity, for at mid-day the light is many times more intense than at morning or evening, and during the summer it is likely to be more intense than during the winter. These considerable variations in quality and intensity, in connection with the fact that no human experimenter is able to control them accurately, render the sun unsatisfactory as a source of stimuli for the *quantitive* study of light vision. For *qualitative* investigations it is invaluable because of its naturalness.

In naturalness the sun is preferable to all other sources of stimuli; with respect to controllability and constancy it is surpassed by several artificial sources; and in measurability it is as satisfactory as most other sources. If we could devise satisfactory ways of controlling the quality and intensity of sunlight, it obviously would be ideal for the study of vision.

b. Nernst lamps.—The Nernst glower yields a light which is qualitatively and intensively fairly satisfactory for the study of vision. In quality its light, although less like daylight than that of the open arc, is about as satisfactory, all things considered, as that of any artificial source at present available.[1] It, therefore is likely to prove reasonably natural. Its spectrum is continuous, but rather weak in the violet.

The quality and intensity of the Nernst light is to a high

[1] Progress in the art of artificial illumination is so rapid that what is written to-day may be misleading to-morrow. The statements in this report must be read with the understanding that they were true in the year 1910.

degree controllable by the experimenter. On a well controlled circuit, the life of the Nernst glower is six hundred hours for the direct current and eight hundred hours for the alternating current, regardless of position. During the first two or three hundred hours of the life of a "glower" the intensity of the light yielded by it diminishes rapidly (10-15 per cent). A new lamp should be allowed to burn for several hours before being used for quantitative experiments. During the mid-life period of the Nernst it is possible, by accurately regulating the current, to obtain light which is fairly constant in quality and intensity. As a rule it is desirable to mount the Nernst glower separately in order to avoid the influence of the "heating coils" upon the quality of the light.

Measurements of quality and intensity can be made as readily with this lamp as with any other source of light. The use of from one to six glowers, according to the demands of the investigation, renders intensities of from 50 to 500 c. p. obtainable.

As compared with the sun, the Nernst lamp is somewhat less satisfactory in respect to the naturalness of its light, vastly more satisfactory with respect to controllability and constancy, and equally satisfactory with respect to measurability. It is, of course, to be noted that it can not be used when high intensities of illumination are required. For more than five hundred candle power the sun and the electric arc are more practicable sources of light.

On the whole the Nernst glower commends itself to us as highly satisfactory for experiments on vision and for the photic stimulation of lower organisms. Its adaptability, stability, and efficiency are important considerations. For *quantitative* work on vision, with stimuli of medium intensities, it certainly is preferable to the sun and to most artificial sources of light.

For detailed information concerning the performance, characteristics of light, prices, etc., of this glower we refer the reader to the Bulletins of the Engineering Department, National Electric Lamp Association of Cleveland, Ohio, to "Lectures on illuminating engineering," Baltimore, The Johns Hopkins Press. 1911, vol. 1, p. 72, to "The Standard handbook for electrical engineers" (2d ed., 1908), and to various publications of the Nernst Lamp Company, Pittsburg, Pa.

c. Tungsten lamps.—This form of incandescent lamp yields a light, which, under favorable conditions, is as satisfactory qualitatively as that of the Nernst glower. In naturalness it may therefore be classed with the latter. The lamp is highly efficient and its light reasonably well fulfills the requirements of controllability, constancy, and measurability. The normal life of the tungsten is about eight hundred hours, and during this period the intensity of the light diminishes by eight to ten per cent. This decrease is barely half that of the Nernst. With the aging of its filament, the quality of the tungsten light changes somewhat, but not so markedly, it would seem, as that of the carbon incandescent. The filament is brittle when cold and there is considerable risk of breakage in moving the lamp quickly, but when hot it is tough and flexible and the lamp may be handled safely.

For the quantitative study of light vision we recommend the tungsten incandescent lamp (1) because it, as nearly as any other reasonably convenient artificial source, meets the requirements of naturalness, controllability, constancy, and measurability, (2) because it is efficient, inexpensive, easily cared for, and adaptable, and (3) because, when carefully seasoned and used, its quality and intensity do not vary greatly during the mid-life period.

It should be emphasized that, in contrast with the Nernst, the tungsten lamp exhibits a steady decrease in intensity during its life, instead of relatively rapid decrease at the beginning and end of the life-history and constancy during the mid-life period. This fact renders the Nernst lamp preferable to the tungsten for certain purposes.

Valuable information concerning tungsten lamps,[1] which are now manufactured in a great variety of forms, may be obtained from the Bulletins of the National Electric Lamp Association, No. 6A, No. 6B, No. 6D, No. 6E. These Bulletins may be obtained from the Engineering Department of the National Electric Lamp Association, 4411 Hough Avenue, Cleveland, Ohio. R. W. Hutchinson's "High-efficiency electrical illuminants and illumination," New York, 1911, also is useful.

d. Carbon incandescents.—Of the numerous forms of carbon

[1] The tantalum lamp has seemed to us less satisfactory for our purposes than the tungsten, and detailed consideration of it is therefore omitted.

incandescent lamps, the metallized filament lamp is, for our purposes, the most satisfactory. All carbon lamps yield a yellowish light, which tends to become reddish with age, and they exhibit a greater sensitiveness to fluctuations in current than does the tungsten. In neither quality nor intensity are carbon lamps as constant as tungsten and Nernst lamps. We recommend them only in case the other lamps are not available, or, for some reason, prove impracticable.

e. Carbon arcs.—The carbon arc as a source of light is valuable for experiments on vision which require high intensities. For all other purposes the Nernst and the tungsten are preferable. The crater of the arc yields a light whose spectrum is continuous and whose quality approaches sunlight more nearly than that of any other artificial light. The light of the arc itself, as contrasted with that of the crater, presents a banded spectrum which is especially intense in the violet. Intensity variations are almost always great, except in the crater of the arc, and it is difficult to control them. These defects render this form of lamp slightly, if any, more satisfactory than the sun for the quantitative investigation of *light* vision. It should be stated, however, that the arc lamp is eminently satisfactory when the light from the crater alone is used.

The faults mentioned above are at their minimum in lamps of the "baby arc" family, to which belong the Siva arc, the Lilliput, the Midget, and other lamps. They are small carbon arcs, used chiefly in Europe, which under high pressure furnish an excellent quality of light in fairly uniform intensity. Unfortunately, none of these arcs burns well out of the horizontal position.

f. Acetylene lamps.—The quality of acetylene light is fairly satisfactory, but its intensity is with difficulty kept constant. In the absence of electricity, this form of lamp may be used to advantage for many tests of vision.

g. Gas lamps.—The light is likely to be over-strong in orange and yellow. Under carefully chosen conditions, in the absence of electric lights, incandescent gas lamps may be used for tests of vision.

h. Oil lamps and candles.—Like the light of the gas flame, that of oil lamps and candles is likely to be over-strong in yellow. With respect to constancy, controllability, and measur-

ability, both oil lamps and candles constitute fairly satisfactory sources, provided the intensity of light needed is not high.

Tables 1 and 2 [1] present data concerning the color- and intensity-values of the sources of light which are most commonly used. It is needless to state that these data are not highly accurate.

TABLE 1

COLOR OF COMMON ILLUMINANTS

Illuminant	Color
Acetylene	Nearly white
Arc light (enclosed)	Bluish white to violet
Arc light (open)	White
Candle	Orange yellow
Carbon incandescent (below voltage)	Orange to orange red
Carbon incandescent (normal voltage)	Yellowish white
Gas light (open flame)	Yellowish white to pale orange
Gem metallized filament incandescent	Nearly white, slightly yellowish
Kerosene lamp	Orange, slightly yellowish
Nernst lamp	White
Mantle burner	Greenish white
Sky light	Bluish white
Sun (high in sky)	White
Sun (near horizon)	Orange red
Tantalum	White
Tungsten	White

TABLE 2

INTENSITY OF LIGHT SOURCES IN CANDLE POWER PER SQUARE INCH OF ILLUMINATING SURFACE

Source	Intrinsic brilliancy	Remarks
Opal shaded lamps (electric)	½ to 2 c. p.	
Frosted incandescent lamps	2 to 5	
Candle	3 to 4	
Gas flame	3 to 8	Varies widely
Oil light	3 to 8	
Mantle burner	20 to 25	Probably too low
Acetylene flame	75 to 100	
Enclosed arc lamp	75 to 200	Depending on globe
Incandescent lamps (carbon)		
Carbon 4.0 watts per candle	300	
Carbon 3.5 watts per candle	375	
Carbon 3.1 watts per candle	480	
Gem metallized filament incandescent, 2.5 watts per candle	625	
Tantalum incandescent	750	
Tungsten incandescent	1,000	
Nernst lamp (bare)	800 to 1,000	
Open arc lamp	10,000 to 100,000	(200,000 in crater)
Sun on horizon	2,000	
Sun at 30° elevation	500,000	
Sun at zenith	600,000	

[1] These tables are taken from "Data on illumination." Bulletin of the Engineering Department of the National Electric Lamp Association, No. 7, June 1, 1907.

i. Recommendations concerning sources for tests of LIGHT *vision.*—In view of the above considerations, and a multitude of others which space limitations force us to omit, we offer the following recommendations concerning sources of stimuli for the investigation of light vision.

(1) That for all strictly accurate quantitative work the tungsten, or the Nernst, be used, in its mid-life period, and on a well-regulated circuit.

The lamps should be burned on a rheostat circuit at a voltage slightly below the optimal (2–3 volts for 110 volt direct current lamp), so that as the efficiency diminishes with age (as is shown by frequent photometric tests), an increase in voltage will serve to maintain the lamp at its standard efficiency. This precaution, and method of obtaining a constant intensity, is especially advantageous in connection with experiments which must be continued for weeks and in the midst of which it is inadvisable to change lamps.

Attention is called to the following important points in connection with the use of tungsten and Nernst lamps. (a) The lamp should be well seasoned and in perfect condition; (b) it should have been burned under constant pressure for from ten to one hundred hours, according to the type, before being employed in quantitative experiments; (c) the current should be carefully regulated during use of lamp; (d) by frequent metric tests the experimenter should discover variations in the quality or intensity of the light yielded by a lamp, and he should replace it as soon as he detects marked changes, such for example as those indicating the end of life.

(2) That the sun be employed as a source for check or comparison experiments, in order that the normal reaction of the animal shall be observed with certainty. That experiments with sunlight or daylight be performed at the same hour each day, preferably toward noon, and under as nearly identical atmospheric conditions as are obtainable.

(3) That in case of the inaccessibility of an electric current, acetylene or gas be used under carefully controlled conditions.

(4) That for qualitative, as distinguished from quantitative, experiments, sunlight or daylight be employed in the study of *light* vision.

2. Measurements of stimulus

Measurement of the photic stimuli employed in a quantitative study of vision is essential. Only on the basis of exact statements concerning the values of stimuli can the conditions of an observation be reproduced and the reliability of the observation determined.

Two varieties of measurement are available at present for students of visual reactions. They may be designated as (a) the psycho-physiological, and (b) the physical. As one of the most important examples of the former variety of method we have photometry; as a typical example of the latter method, radiometry.

Photometry involves optical comparison of two illuminated areas, one of which is the standard and the other the compared area. This comparison results in a measurement of the visual stimulus in terms of a conventional unit of light (candle-power, hefner, carcel). The method, in all of its forms, is dependent upon the visual capacity, training, and the special skill of the observer who attempts to use it. For this reason, and others only less important, it is usually desirable to supplement photometric measurements of photic stimuli by measurements of their value in terms of energy. Hence the pertinence of physical methods.

Determination of the value of photic stimuli in terms of heat units by a radiometric procedure has proved feasible. Radiometry yields a measurement which is relatively independent of the visual peculiarities of the observer, and it therefore supplements in an invaluable manner the results of photometry.

a. Photometry of white light stimuli.—It is difficult to obtain accurate measurements of the stimuli employed in investigations of vision which are conducted in daylight. But whenever a dark-room is used, it is possible to employ photometry profitably. We shall limit our recommendations to dark-room measurements.

The Lummer-Brodhun prism photometer,[1] with a standardized

[1] Brief descriptions and prices of the various forms of the Lummer-Brodhun photometer may be found in the "Photometrische Apparate" catalogues of Franz Schmidt and Haensch, Berlin, Germany. The catalogues may be obtained, and importations satisfactorily made, through Mr. James G. Biddle, 1114 Chestnut Street, Philadelphia, Pa. See also Stine, W. M., "Photometrical measurements," New York, 1904.

tungsten lamp as a source,[1] is recommended for use in connection with the methods of investigating vision whose adoption, subject to all possible improvement, we urge.

The Hefner amyl-acetate lamp provides a photometric standard which is unsatisfactory because of its yellowish color. In all cases, qualitative (color) difference between the standard and the photometered light should be avoided.

If an electric standard is inaccessible, a standard candle may be employed.

In case the Lummer-Brodhun photometer is not available, some cheaper form, (for example, the Bunsen) [2] may, with care, be used to advantage. It is, however, high time that we students of animal behavior realized that the quantitative investigation of a problem can be made worth while only by the employment of the best methods of measurement which are available. The employment of the most precise instruments for the investigation of vision can not be too strongly urged.

For the determination of extremely low intensity values (threshold stimuli) in limited spaces, we recommend that the Lummer-Brodhun screen be used in connection with a rotating sector. By means of the latter the intensity of the standard may be reduced to any desired amount.

We further recommend that the values of lamps be given in candle power (c. p.) and the value of stimuli in candle meters (cd. m.).

b. Radiometry of white light stimuli.—"Light is not a simple multiple of radiation, but a complicated function of its quality, quantity, and duration." [3] For this reason, measurements of the energy of light are of uncertain value in physiology and

[1] Standardized electric lamps may be purchased of the General Electric Company, Harrison, New Jersey, or of the Electrical Testing Laboratories, 80th Street and East End Avenue, New York, and the latter will make special photometric determinations, at rates obtainable on request. Problems relating to units of measurements and standards may advantageously be referred by the inexperienced investigator to the National Bureau of Standards, Washington, D. C., S. W. Stratton, Director.

[2] For an excellent discussion of "photometric units and standards" and "the measurement of light" the reader is referred to *Lectures on illuminating engineering*, Baltimore, The Johns Hopkins Press, 1911, vol. 1, pp. 387-506. These two volumes of lectures present also invaluable information concerning the characteristics of sources of light.

[3] Nutting, P. G. The Luminous equivalent of radiation. Bull. U. S. Bureau of Standards, 1908, vol. 5, p. 262.

psychology. Nevertheless, it seems highly desirable that energy measurements be made to supplement our photometric data.

Of the various radiometric devices and methods [1] which are at our command, the radiomicrometer and the selenium cell have been at least partially adapted to our needs and tested in investigations which are now in progress.

Concerning the former instrument Coblentz writes: "The radiomicrometer is essentially a moving coil galvanometer having a single loop of wire with a thermo-junction at one end. This instrument was invented independently by d'Arsonval [2] and by Boys.[3] The former used a loop, one part of which was silver and the other was of palladium. The latter used a junction of bismuth and antimony, which was soldered to a loop of copper wire.

"The sensibility of the Boys instrument was given as $\frac{1}{10\,000\,000}$ to $\frac{1}{94\,000\,000}$ of 1°. From subsequent work with other radiation meters in which this high degree of sensitiveness has never been attained, it would appear that the sensibility of the radiomicrometer was overestimated." [4]

Professor G. H. Parker is at present using for the measuring of photic stimuli, as applied to organisms without highly developed visual organs, the Boys radiomicrometer essentially as it is described by Adams.[5] As he later will publish a description of the method, with the results of his experiments, we need add merely a comment.

The radiomicrometer is certain to prove useful in connection with studies of *light* vision. It is not sufficiently sensitive to enable us to measure directly extremely weak stimuli (threshold or approximations thereto), and it can not be used at all unless a highly stable base is available.

The selenium cell [6] has been classed among the "selective

[1] Coblentz, W. W. Instruments and methods used in radiometry. Bull. U. S. Bureau of Standards, 1908, vol. 4, pp. 391–460.

[2] d'Arsonval. Soc. Franc. de Phys., 1886, pp. 30, 77.

[3] Boys, C. V. Phil. Trans., vol. 180, pp. 159.

[4] Coblentz. Loc. cit., p. 395.

[5] Adams, J. M. The transmission of roentgen rays through metallic sheets. Proc. Amer. Acad., 1907, vol. 42, p. 673 ff.

[6] Pfund, A. H. The electrical and optical properties of metallic selenium. Physical Review, 1909, vol. 28, pp. 324–336.

Stebbins, Joel. The color-sensibility of selenium in cells. Astrophysical Journal, 1908, vol. 27, pp. 183–187.

radiation meters."[1] It is extremely sensitive, as compared with the radiomicrometer, and it has the great advantage of being serviceable where only a moderate degree of stability is possible. In an adaptation to our needs made by Professor A. H. Pfund, the selenium cell is now being tested by Professor Watson. In view of the results obtained, the apparatus promises well for measurements of both light and color stimuli. It provides a convenient means of equating any two *light*, *size*, or *form* stimuli with respect to intensity of *light*. The measurements may be made quickly as well as accurately. Subsequently, if it prove desirable, determinations of the photometric values of the stimuli may be made. In as much as it promises to be especially valuable in connection with studies of color vision, further description will be reserved for our section on the measurement of chromatic stimuli.

We recommend that photometric measurements of *light* stimuli be supplemented by radiometric measurements, obtained by the use of some form of radiomicrometer, selenium cell, or other heat meter.

3. Application of stimulus to animal

a. Common methods.—There are almost as many methods of applying achromatic stimuli as there are investigators. Many of the methods in common use are excellent for qualitative investigations, but few, if any, are satisfactory for quantitative work.

The type of method[2] most in evidence depends upon the reflection of varying amounts of light (natural or artificial) from the surfaces of paper, wood, glass or metal. White, grey, and black papers and cardboards are extensively used as visual testing materials. They are unsatisfactory in that it is extremely difficult to describe accurately the conditions of stimulation and almost as difficult to control or modify them.

b. Method recommended.—The method which it is our purpose to describe in Section III as a standard procedure for testing

[1] Coblentz. Loc. cit., p. 454.

[2] Graber, V. Grundlinien zur Erforschung des Helligskeits und Farbensinns der Tiere. Prag und Leipzig, 1884.

Small, W. S. An experimental study of the mental processes of the rat. Amer. Jour. Psychol., 1899, vol. 11, p. 80, p. 133; 1900, vol. 12, p. 206.

Kinnaman, A. J. The mental life of two *Macacus rhesus* monkeys in captivity. Amer. Jour. Psychol., 1902, vol. 13, pp. 98–148, 173–218.

the light vision of an animal involves the presentation, in a dark-room, of two visual areas which differ only in intensity of illumination. These visual areas are obtained by illuminating semi-opaque glass plates with a standard source which is placed on the opposite side from the animal. The chief advantages of this general method of presenting *light* stimuli, in contrast with the reflection methods which are usually employed by psychologists and often by physiologists, are three: (1) The visual stimulus can be controlled accurately with respect to intensity; (2) it may be photometered and radiometered with accuracy; (3) other visual factors than intensity of *light* may be excluded or kept constant; (4) other factors than the visual may be excluded or controlled more certainly and satisfactorily than in the case of most methods.

While recognizing the value of the commonly employed methods as means of obtaining certain kinds of information about the *light* perceiving capacities of an organism, we wish to urge the use of more accurately controllable and measurable stimuli in all quantitative investigations.

II. METHODS OF INVESTIGATING VISUAL SIZE, FORM, AND DISTANCE PERCEPTION

1. Size perception

Students of vision have investigated the perception of size, with few exceptions, by means of crude methods; and the results of such quantitative studies as have been made by excellent methods can not be compared because of fundamental differences in experimental procedure. It has been our purpose to devise a method which shall be adaptable; which shall yield accurate quantitative results; and which shall render possible and profitable the direct comparison of results obtained with different animals.

Size perception, or the ability of an organism to detect differences in size, has been studied chiefly by "the method of the discrimination of objects" (boxes, blocks, balls, glasses, etc.) on the basis of difference in size. Thus, for example, an animal is tested by being required to select the smaller of two boxes in order to obtain the reward of food. The method has many advantages—like all other reasonably well planned quali-

tative methods—but it does not and can not, as ordinarily used, enable us to determine with accuracy the limits of an animal's ability to perceive difference in size. In other words, it does not lend itself to the thorough investigation of size perception.

We wish to recommend a method whose chief defect, apparently, is unnaturalness, and whose prominent advantages are accurate controllability and describability.

This, in brief, is the method. In a dark-room, and by means of the apparatus described below in Section III, two visual areas are simultaneously exposed to view. These areas differ only in size, and with respect to this character they are under the control of the experimenter. Differences of the compared stimuli as to form, color, light, texture, position, odor, which exist and in varying degrees influence the results obtained by common methods, are largely or wholly excluded by our standardized apparatus.

2. Form perception

The statements made concerning size perception apply also to form perception. The procedure recommended, and for which the necessary apparatus is described in Section III, p. 17, involves the use of standard plates in which are cut openings in the shape of circles, hexagons, squares, and triangles.

3. Distance perception

Few students of vision have considered the problem of distance perception in animals.[1] This is rather because of the difficultness of the task than because the problem lacks interest or importance. We have made some preliminary attempts to devise a satisfactory method of dealing with the subject, but we are not prepared at present to make recommendations. It seems to us especially important that this visual factor be studied in connection with *color* vision.

[1] Thorndike, E. L. The instinctive reactions of young chicks. Psychol. Review, 1899, vol. 6, p. 284 ff.

Waugh, K. T. The rôle of vision in the mental life of the mouse. Jour. Comp. Neurol. and Psychol., 1910, vol. 20, p. 572 ff.

Yerkes, R. M. Space perception of tortoises. Jour. Comp. Neurol. and Psychol., 1904, vol. 14, p. 17 ff.

III. DESCRIPTION OF STANDARDIZED APPARATUS AND METHOD FOR THE STUDY OF *LIGHT*, SIZE, FORM, AND DISTANCE PERCEPTION

Without wishing to dictate to any experimenter either details of apparatus or methods of experimentation, we present the following description of a mechanical device and of an experimental procedure which in our experience yield excellent results.

1. *Light* or "brightness" apparatus

This apparatus consists of three chief parts: (1) A light box, fig. 1, A; (2) an experiment box suited to the animal to be tested, fig. 4; and between the two (3) a stimulus adapter, fig. 3, by means of which two illuminated areas are simultaneously exposed to view.

The wooden light box, A, is divided into two compartments, C and D, by the partition, B. The sides, ends, and partition of the box are $\frac{7}{8}$ inch planed and seasoned lumber. The inside dimensions are as follows: length, 3 meters; width (between sides) 52 cm.; depth, 30 cm. The bottom is $1\frac{3}{4}$ inch planed stock, dowelled and glued. It is made 2 feet longer than the box, as a provision for the support of the experiment box.

To the middle partition of the box are hinged two lids, E and F, of $\frac{7}{8}$ inch lumber.[1] The edges of the box and lids are grooved and rabbeted. When the lids are closed the two compartments, C and D, are light-tight with respect to one another.

Two cast-iron carriages, G and H, carry incandescent lamps which serve as sources of photic stimuli. Each carriage rides on a pair of steel tracks, IJ and KL, placed on the floor of its compartment.

To the floor of each compartment is attached a Starrett (Athol, Mass.) steel tape, M and N, from which the position of the source of light may be read directly in millimeters.

In order that daylight, instead of artificial light, may be used when it seems desirable, a hole 12.7 cm. in diameter is cut in the end of each compartment. These holes are fitted with Aubert diaphragms as shown at O and P, fig. 1.

The completed apparatus carries a system of ball-bearing pulleys, cords, and levers (not shown in fig. 1) by means of

[1] The drawing for fig. 1 was made while the lids were hinged to the sides. Later it was found desirable to hinge them to the middle partition. We recommend the latter form of construction.

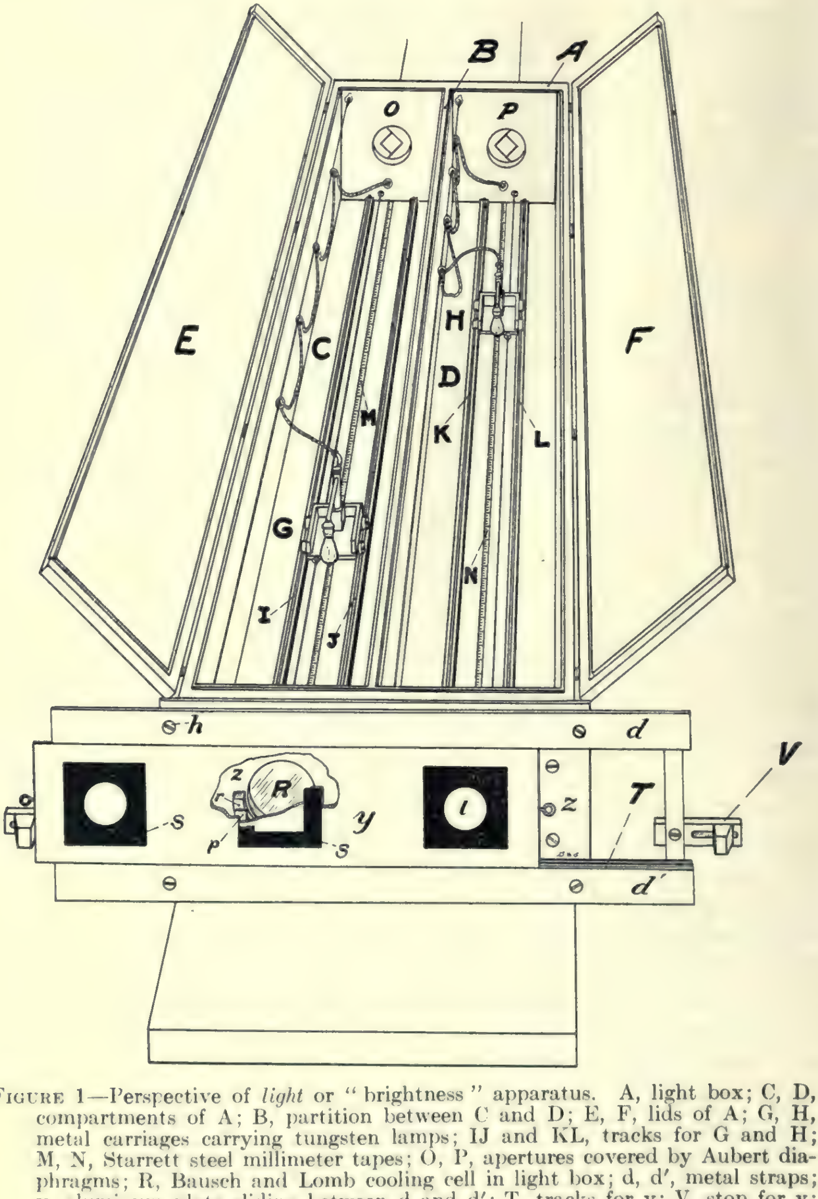


FIGURE 1—Perspective of *light* or "brightness" apparatus. A, light box; C, D, compartments of A; B, partition between C and D; E, F, lids of A; G, H, metal carriages carrying tungsten lamps; IJ and KL, tracks for G and H; M, N, Starrett steel millimeter tapes; O, P, apertures covered by Aubert diaphragms; R, Bausch and Lomb cooling cell in light box; d, d′, metal straps; y, aluminum plate sliding between d and d′; T, tracks for y; V, stop for y; z, steel plate bolted to wooden end of light box; h, screws attaching y to z; s, s, standard brass stimulus plates; p, brass frame about aperture in y; r, hard rubber ring screwed to p.

which the experimenter may shift the lamp carriages without approaching the light box or experiment box. Fig. 2 presents

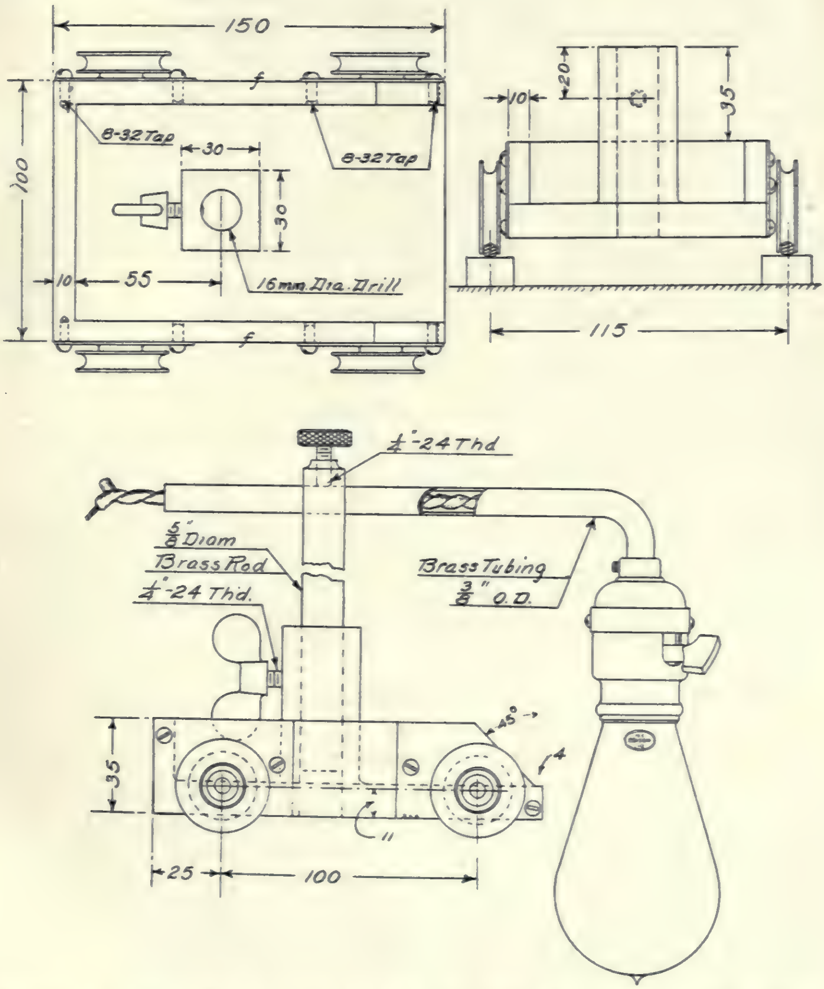


FIGURE 2—Construction drawings for lamp carriages. Dimensions, unless otherwise specified, are given in millimeters. Castings for the base of carriage may be obtained through the writers.

construction drawings of the lamp carriages. The rollers were obtained from Francis Keil & Son, New York. They are known as ball-bearing sheaves no. 17074. These rollers neatly fit a

track made by soldering, or screwing, a 3-16 inch diameter coppered iron rod to the broader side of a $\frac{1}{2}$x$\frac{3}{4}$ inch coppered iron bar. The weight of this track is sufficient to hold it in position, but it may advantageously be lightly secured to the floor of the box at two points. If the rod be screwed, instead of soldered, to the bar, it is well to insert a strip of $\frac{1}{8}$ inch felt between them, throughout their length, to diminish the noise of the moving carriage.

Against the experiment box end of each compartment of the light box is placed a water cell (R, fig. 1) to serve as an adiathermal screen. The cooling cells used by Bausch and Lomb in their projection lanterns prove satisfactory. They are circular metal cells of at least 11 cm. diameter with a water space of 4.8 x12.5 cm.

The stimulus adapter is a device by means of which the experimenter is able to regulate the size, form and position of the visual stimuli. It appears as the front (end) of the light box when the experiment box, fig. 4, is removed. In fig. 1 it is shown as a unit and in fig. 3 construction drawings are provided.

A steel casting $\frac{3}{8}$ inch thick, carefully planed, and firmly bolted to the light box is shown at z, fig. 1. This sheet of steel contains two circular apertures, 10 cm. in diameter, through which the light passes from the light box to the experiment box.

To the metal plate z, the stimulus adapter is attached by four bolts, one of which is labelled as h in fig. 1.

The essential parts of the stimulus adapter are: (1) a metal frame composed of the straps d and d′, fig. 1, and the two vertical straps, e, fig. 3. The inner edges of the horizontal straps are rabbeted and into them is fitted (2) the aluminum plate y, which slides smoothly on a pair of tracks, k, fig. 3, which are screwed to d and d′. Y moves on six rollers, m, n, o, fig. 3. It contains three windows, each 12 cm. square, located 27 cm. apart (center to center). These windows receive the standard stimulus plates described below. (3) Back of each window is attached by four small screws a square frame of 1-16 inch sheet brass, p, fig. 1, 9.3 cm. on its inner edges and 13.3 cm. on its outer edges. A flange is thus created which serves to hold; in each window, (4) a brass plate, s, fig. 1, 12x12 cm. and 1-16 inch thick. This plate contains an accurately cut opening.

It is held firmly in position by two screws through diagonally opposite corners of the metal frame p. At any time by loosening the screws (v. fig. 3) the experimenter may readily remove a plate and replace it by another with an opening of different

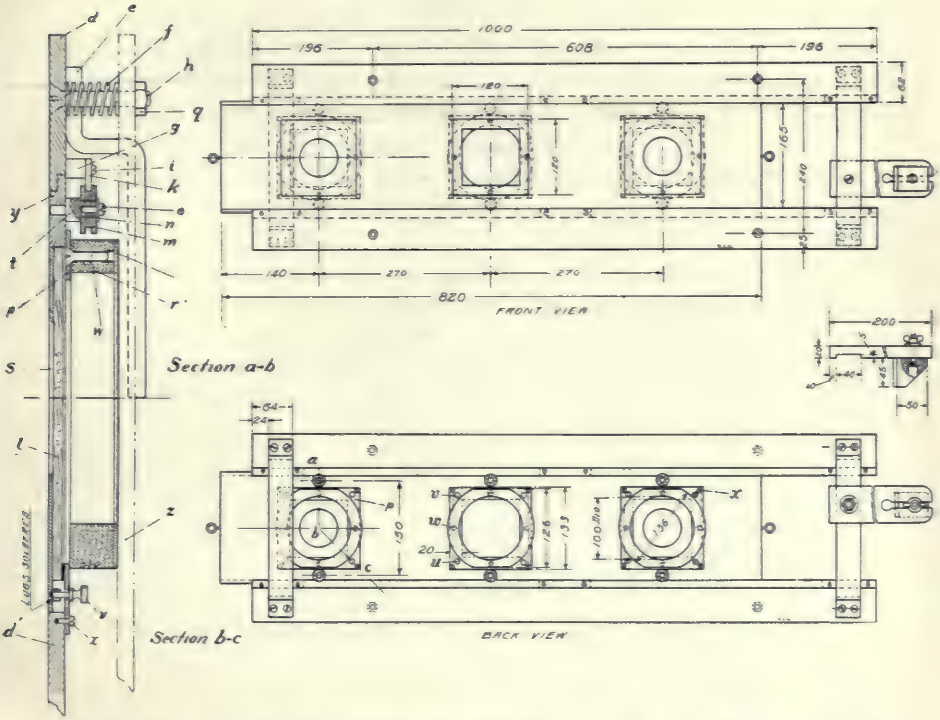


FIGURE 3—Construction drawings for stimulus adapter. Dimensions in millimeters. *Front view. Back view*; p, brass plate framing the aperture in aluminum plate y; u, head of one of the two screws which serves to hold opal glass plate firmly against standard brass stimulus plate (the other screw is in the corner diagonally opposite); v, one of the two screws which serves to hold standard stimulus plate in position (the other is in the corner diagonally opposite); w, screws attaching hard rubber ring to p; a-b, and b-c indicating sections. *Section a-b*; d, d′, metal straps with rabbeted edge for aluminum plate y; e, strap connecting d and d′; f, steel spring on h, bolt tying d to z; g, brass collar for screw, i, which attaches brass track, k, to d; m, n, o, t, parts of brass roller which enables y to move on track; p, brass plate framing aperture in y; r, hard rubber ring about aperture; w, screw attaching r to p; s, standard brass stimulus plate. *Section b-c*; 1, plate of flashed opal glass.

size or form. The set of standard plates is fully described in connection with table 3, p. 23. Between the frame p, and the standard plate, s, is inserted a piece of opal flashed glass, l.

This serves as a diffusing surface and constitutes the stimulus area. It is held against the brass plate, s, by two screws, u, fig. 3. (6) To the metal frame p is screwed from the front at w, fig. 3, and three other points, a ring of hard rubber, n, 1.8 cm. thick, 2 cm. wide with a circular aperture 10 cm. in diameter. To this rubber ring is glued a ring of piano felt about ½ cm. thick. These hard rubber and felt rings serve to fill in the space between the metal front, z, of the light box, and the aluminum plate, y, which carries the stimulus plates. (7) By means of the bolts, at h and corresponding positions in the three other corners, the stimulus adapter may be brought into the necessary proximity to z to prevent light from passing from one stimulus opening to the other, between z and y. Each of the four bolts (h) at the corners of the stimulus adapter carries, between z and y, a coiled spring, f, fig. 3, which serves to press y away from z. By putting the proper amount of pressure on the four springs the experimenter can so adjust the surfaces of the rings of piano felt to the planed front of the steel plate z that the light can not pass between the two, while, at the same time the surfaces may be moved over one another freely whenever it is necessary to move y. (8) At either end of the frame of the stimulus adapter a stop, V, fig. 1, is attached so that y shall not run beyond the track T.

The most important part of the "brightness" apparatus is the set of accurately made brass stimulus plates which is briefly described in table 3.

The set, as used by us at present, consists of twenty-six plates. Others may readily be added as they are needed. These plates fall into three groups: (1) the light perception plates—three plates with a 6 cm. circular opening and three with a 5 cm. circular opening. This provides a plate for each of the three windows of the stimulus adapter with a diameter of either 5 or 6 cm. (2) The size perception plates—this group consists (including those of group 1) of plates with circular openings which, between 6 cm. and 5.5 cm., differ by 1 mm. in diameter, and between 5.5 cm. and 3 cm. by 5 mm.[1] (3) The form perception plates—there are in this group four plates whose open-

[1] In case an animal should prove able to discriminate slighter differences in size than 1 mm. it would be necessary to cut additional plates, but this may readily be done.

ings differ in form while being equal in area. They are (a) the 6 cm. circle; (b) a hexagon 3.299 cm. on the side; (c) a square 5.317 cm. on the side; (d) and an equilateral triangle 8.081 cm. on the side. The area of each opening is 28.2743 sq. cm. In

TABLE 3

Description of Standard Stimulus Plates. Each Plate is a 12 cm. square of 1-16 inch Acid Blackened Brass, Containing an Accurately Cut and Centered Opening.

Description of Openings

Use	Form	Diameter or side	Area	Number of plates needed
To test				
Light	Circle	6.000 cm.	28.2743 sq. cm.	3
Perception	"	5.000 cm.	19.6350 sq. cm.	3
	The 6 and 5 cm. circles (as above) and			
	Circle	5.900 cm.	27.3397 sq. cm.	1
	"	5.800 cm.	26.4208 sq. cm.	1
	"	5.700 cm.	25.5176 sq. cm.	1
To test	"	5.600 cm.	24.6301 cm. sq.	1
Size	"	5.500 cm.	23.7583 sq. cm.	2
Perception	"	4.500 cm.	15.9043 sq. cm.	1
	"	4.000 cm.	12.5664 sq. cm.	1
	"	3.500 cm.	9.6211 sq. cm.	1
	"	3.000 cm.	7.0686 sq. cm.	1
	Circle	6.00 cm.	28.2743 (as above)	
	Hexagon	3.299 cm. (side)	"	1
	Square	5.317 cm. "	"	2
To test	Equil.			
Form	triangle	8.081 cm. "	"	2
Perception				
	Openings inscribed in 6.000 cm. circle			
	Hexagon	3.000 cm.	23.382	1
	Square	4.243 cm.	18.003	2
	Equil.			
	triangle	5.196 cm.	11.691	2
			Total..................	26

addition to the above there are provided three forms which are inscribed in the aperture of the 6 cm. circle. They are (a) a hexagon 3.000 cm. on the side; (b) a square 4.243 cm. on the side; and (c) an equilateral triangle 5.196 cm. on the side.

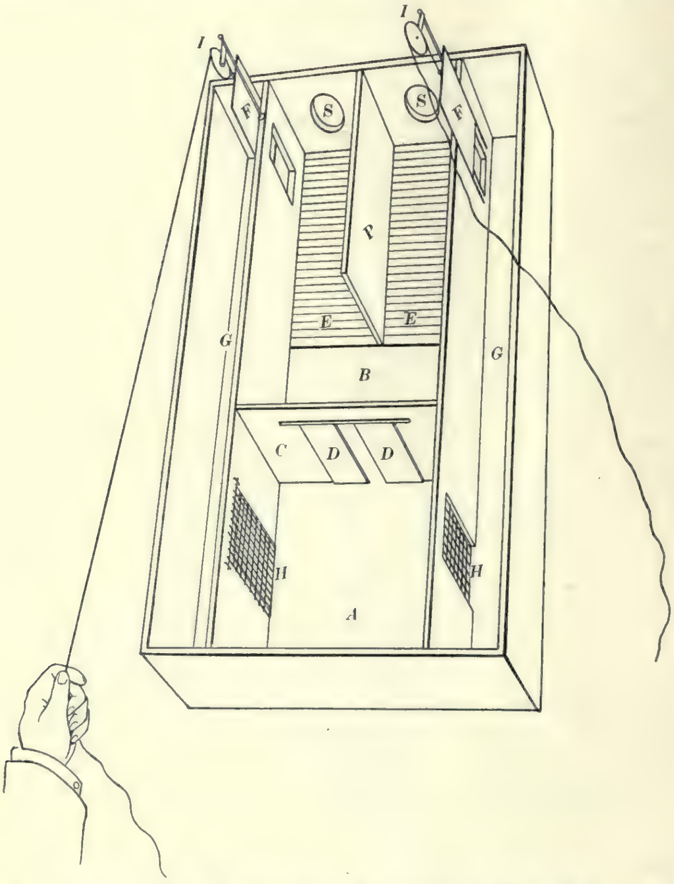


FIGURE 4—Experiment box for brightness apparatus. Planned for kittens. A, entrance chamber; B, discrimination chamber; C, partition; D, D, sliding doors; E, E, electric passage-ways; P, partition; S, S, stimulus apertures; F, F, doors for exits from E, E; I, I, pulleys for cords from I, I; G, G, alleys to A; H, H, swinging doors.

The experiment box differs from the light box and the stimulus adapter in being a variable. The latter are permanent and may be used for the study of any animal. The former, in every case, must be adapted to the animal. The particular experiment box

represented in fig. 4 happens to be one which was made for experiments with kittens. It is extremely simple. The essential parts are the entrance chamber, A; the discrimination chamber, B; a partition, C, between A and B, with a pair of sliding doors, D, D; the identical passage-ways, E, E, the floors of which consist of a slab of slate wound with phosphor bronze wire so that the animal can, when necessary, be given an electric shock; the partition, P, between the electric passage-ways; the stimulus areas, S, S, which during experiments are approximated to the two opal glass plates, of the stimulus adapter; the doors (and doorways), F, F, which remain closed except when the experimenter raises them by means of cords running over the pulleys I, I; the alleys, G, G, which, through H, H, give access to the entrance chamber.

This experiment box is by no means perfect. It is described in this connection merely for the purpose of exhibiting the relations of the several portions of the apparatus.

TABLE 4

List and Approximate Cost of Parts of "Brightness" Apparatus

Light box	$ 25.00
Lamp carriages	6.00
Lamp rods and adjusters	2.50
Aubert diaphragms [1]	
Tracks	5.00
Millimeter tapes (Starrett)	3.00
Cooling cells	10.00
Steel plate for end of light box	8.00
Stimulus adapter	25.00
Standard stimulus plates (each $.75)	19.50
Sheaves, cords, levers	5.00
Experiment box	5.00
Lamps, rheostat, meters	$50.00–100.00
Total cost, exclusive of last item	$124.00

The apparatus can undoubtedly be made, now that the plans have been perfected, for about one hundred dollars.

[1] No estimate of cost of the Aubert diaphragms is given because we have not as yet designed accurate forms thereof.

2. Points of method

The use of the "brightness" apparatus involves a number of important points of method.

The sources of light should be well seasoned tungsten lamps burned on a carefully regulated circuit, and every precaution should be taken to avoid sources of error from reflection within the light box.

The stimulus area (i.e., the illuminated window of opal glass which is framed by an accurately cut standard plate) should consist of a carefully selected piece of "flashed opal glass."[1] As this glass varies in thickness and contains imperfections, it is absolutely essential that the three windows of the stimulus adapter be fitted with pieces which are as closely matched as possible. Unless great care be exercised in selecting and placing the diffusing glasses the value of the apparatus is certain to be impaired. The lights must be centered with the stimulus windows, and these, in turn, with the openings into the experiment box. It is desirable that the experiment box be so arranged that the photic stimulus may be measured photometrically and radiometrically just as it appears to the animal. Since the value of the method which we are proposing depends upon the comparison of two photic stimuli which differ from one another, in a certain definite amount, with respect to some one character, it is important that every precaution be taken to keep the stimuli separate. This means that the apparatus must in effect be double, the one half being wholly beyond the photic influence of the other.

Discrimination by difference in the degree of illumination of the two passage-ways of the experiment box should be prevented by covering the portions of the passage-ways near the stimulus windows with black velvet. In some cases it is likely to prove necessary to arrange a truncated cone of dead black velvet about each stimulus area. Thus the animal can be forced, in its attempts to discriminate in favor of the right passage-way, to depend solely upon a visual difference in the two illuminated windows.

In its perfect form the "brightness" apparatus should enable the experimenter to present two visual stimuli which differ from

[1] This glass is obtainable through Semon Bache and Company, West and Hubert Streets, New York, at 40 cents per square foot, in cut sizes.

one another in a definite, constant, measured way with respect to some one characteristic, and only one. It should, further, enable him to exclude all secondary and irrelevant sensory criteria by which the discriminative reaction of the animal might be influenced.

Account must be taken, in any accurate visual tests, of the animal's degree of visual adaptation. This must be controlled. It is necessary, therefore, to control carefully, and to measure (a) the illumination of the experiment room between tests, and (b) the intervals of exposure to the influence of stimuli and to general illumination, or the lack thereof. No definite procedure can be recommended because the method must be determined by the characteristics of the animal under observation.

The stimulus adapter must be adjusted, between experiments, noiselessly, and the experimenter must keep himself beyond the range of vision of the animal as much as possible.

In order, then, that reasonably accurate and strictly comparable data concerning the *light*, size, form, and distance perception of animals may be gathered in different laboratories we recommend:

(1) That the simple discrimination method be employed by presenting to the animal simultaneously two photic stimuli, the one of which calls for a positive response and the other for a negative response.[1]

(2) That the following motives, singly or in combination, be considered, and if possible used as conditions favoring the establishment of the proper method of reacting: (a) Escape from the experiment box to a larger, more comfortable, and more natural (home-like) cage; (b) return to companions; (c) the obtaining of food by choice of a certain stimulus; (d) the avoidance of punishment—weak electric shock or other suitable form—for inappropriate reactions.

In our opinion it is desirable, for quantitative investigations, that conditions for the establishment of a habit be chosen with careful consideration of their constancy and controllability.

[1] For full description of the discrimination method and various modifications thereof the reader is referred to the following publications:

Yerkes, R. M. The dancing mouse. New York, 1907.

Watson, J. B. Some experiments bearing upon color vision in monkeys. Jour. Comp. Neurol. and Psychol., 1909, vol. 19, pp. 1–28.

Casteel, D. B. The discriminative ability of the painted turtle. Jour. Animal Behavior, 1911, vol. 1, pp. 1–28.

The matter deserves as serious attention as does the method of presenting the stimuli which are to be discriminated.

(3) That preliminary, problem-defining investigations of vision be carried on with rough and ready methods, and that by means of the "brightness" apparatus systematic and detailed studies be made of the visual capacities of one, or a few, individuals of known life-history. Thus our results may be rendered intensive, accurate, and valuable for comparison with the data on human vision.

(4) That the visual stimuli be presented in an apparatus whose construction is in essentials like the one described.

(5) That light perception be investigated by the presentation of two visual areas (circular in form and either 5 or 6 cm. in diameter) whose photic values are accurately controlled and measured by the experimenter.

(6) That size perception be investigated by the presentation, similarly, of two visual areas whose only constant difference is in size. The stimulus adapter might carry, for example, a 5 cm. circle in the middle window, and a 6 cm. circle in each of the end windows. To the animal there could be presented either 6 cm. circle on right and 5 cm. circle on left, or the reverse, as the experimenter desired. Discrimination could be made to depend wholly upon ability to react to the size difference of the areas.

(7) That form perception be investigated by the presentation of two visual areas whose only constant difference is in form. The form plates have been so chosen that the two forms may be either equal or unequal in area. For example, a square and a circle both measuring 28.2743 sq. cm. may be used; or with the same circle may be used a square which may be inscribed in the circle.

IV. METHODS OF INVESTIGATING COLOR VISION

1. Sources of stimulus

The statements concerning the advantages and disadvantages of sources of light made on pages 4 to 10 apply, with certain qualifications, in this connection. In experiments on color vision we may use either a light capable of yielding, in fair intensity, any of the hues of the solar spectrum, or a light

which, instead, yields a line spectrum. Since the particular method employed to obtain a monochromatic stimulus determines the nature of the source which may be used, it is necessary to make special recommendations for each of the several methods of investigating color vision.

For all color vision experiments it is highly desirable that the source provide a light which is natural (like sunlight in quality) constant, controllable, and measurable.

a. Sun.—Sunlight is perfect as to naturalness (quality) and it certainly should be used extensively for comparison experiments in connection with the use of artificial lights. For accurate quantitative experiments it is at present extremely unsatisfactory because of (a) uncertainty of supply, (b) fluctuations in quality and intensity, and (c) uncontrollability.

b. Nernst lamp.—The Nernst lamp yields a light whose quality is excellent and whose intensity, during the last half of the life of the glower, is fairly constant. What is lost in naturalness by the use of this source, instead of the sun, is more than compensated by the gain in constancy of quality, intensity and controllability. For qualitative experiments the Nernst glower is admirable, if intelligently handled; and for quantitative work it is probably the best source now available. It can not, however, be used for extremely high intensity work.

c. Tungsten lamp.—Of the medium power electric lamps the tungsten appears to be the most satisfactory for investigations of color vision which do not demand stimuli of high intensity.

d. Acetylene.—In the absence of electricity, the acetylene lamp may be used to advantage in the study of color vision.

e. Carbon arcs.—The crater of the open carbon arc yields a light of eminently satisfactory quality, but the remainder of the light is unsuitable for visual work. A well constructed arc-lamp, burning high grade carbons, on a well controlled circuit may be made to supply a large amount of light of satisfactory quality and in fairly constant intensity, provided the crater alone be used.

f. Recommendations concerning sources for tests of color vision.—In general, as sources of light for the study of color vision we recommend the following in order of preference (a) sun, (b) open carbon arc, (c) Nernst, (d) tungsten, (e) acetylene. These sources are not, however, equally suitable for all methods.

Some are especially valuable for low intensities of light; others for high intensities.

As sources which are especially strong in certain portions of the spectrum we recommend:

For red, orange, and yellow: Sun (low); mercury arc; amyl-acetate lamp; sodium flame; combustion gas lamps; oil lamps.

For green: Mercury arc; welsbach.

For blue and violet: Skylight; enclosed carbon arc; mercury arc.

2. Methods of obtaining and applying stimulus

Of the many methods by which chromatic stimuli may be obtained for experiments on vision we shall discuss the values of only those which seem to us practicable in the present state of our knowledge. For convenience of description we have classified the methods under the three rubrics of *reflection*, *transmission*, and *dispersion*. There are in reality, for our purposes, two physical phenomena which yield colored light: selective absorption, and the resolution of white light. The former gives the phenomena of object-color in nature; the latter exhibits itself in the rainbow, and in various ways through refraction and interference. In the case of selective absorption, the chromatic stimulus may come to the eye as reflected light (from the absorbing surface) or as transmitted light, if the medium be partially transparent. The phenomenon of selective absorption appears, in its two forms, in colored papers and colored glasses: the former yield chromatic stimuli by selective absorption and reflection; the latter by selective absorption and transmission.

We shall briefly consider, in turn, each of the three groups of methods.

a. The reflection method.—Here we class substances which, because of their capacity for absorption, reflect only a definitely limited range of wave-lengths. Chief among them, for our special purposes, are colored papers, colored cloths, and oil pigments on opaque substrata.

Colored papers.—These, in various forms, have long been used, and still are extensively employed in the study of human color vision. The most pertinent description of them would seem to be an enumeration of their merits and defects.

Merits: Availability in many colors and saturations (hues, tints, chromas);[1] cheapness, and convenience of handling;

[1] Titchener, E. B. A text-book of psychology. New York, 1910, p. 54.

possibility of ready use in daylight, sunlight, or artificial light; naturalness of colors. The latter would seem to be a very important consideration, for colored objects in nature are seen in white light as colored surfaces, as are colored papers when used in daylight or sunlight.

Defects: Inconstancy of qualities in successive sets of the same manufacture; rapid fading under the influence of strong light; impossibility of changing quality, except by the substitution of another paper; extreme difficulty and inaccuracy of measuring either the wave-length or the intensity of the reflected light; impossibility of getting any desired quality of light.

As compared with spectral chromatic stimuli those obtained from colored papers are unsatisfactory (a) because they can not be well controlled with respect to color, saturation, and intensity (the number of papers is finite); (b) because they are not reliable either from day to day or from set to set, and (c) because it is practically impossible to describe them accurately as to color, saturation, and intensity.

In view of this list of defects, it seems impossible that papers should be extensively used in the future for quantitative investigations of color vision. They are invaluable for rough preliminary tests, for class-experiments, demonstration experiments, and, indeed, for all qualitative investigations which do not demand complete control and accurate description of the chromatic stimuli employed.

The characteristics of psychologically satisfactory colored papers are enumerated by Titchener. [1] In the opinion of the writers, spectral light is preferable even to the stimuli from papers which fulfill all of Professor Titchener's requirements. Our reasons for holding this opinion will appear in our description and discussion of the method which we recommend for the quantitative investigation of color vision.

A number of investigations of color vision in animals,[2] prominent among which are those conducted under the direction of

[1] Titchener, E. B. Experimental psychology, vol. I, part II, p. 14.

[2] Kinnaman, A. J. Mental life of two *Macacus rhesus* monkeys in captivity. Amer. Jour. Psychol., 1902, vol. 13, p. 43.

Cole, L. W. and Long, F. M. Visual discrimination in raccoons. Jour. Comp. Neurol. and Psychol., 1909, vol. 19, p. 657.

Samojloff, A. and Pheophilaktowa, A. Ueber die Farbenwahrnehmung beim Hunde. Centralb. f. Physiol., 1907, Bd. 21, S. 133.

Professor E. C. Sanford at Clark University, indicate both the serviceability and disadvantages of colored papers in this kind of work.

In order of preference, we mention the following sets of papers:

1. The Hegg colored papers. (Pfister und Streit, Math. physikalische Werkstatte, Bern, Switzerland).—These are mixtures of oils on paper yielding the hues red, yellow, green, and blue. These hues are claimed to be equal in intensity and saturation for the human eye. The set is useful as a means of ascertaining, in a preliminary survey, whether an animal readily discriminates two hues which for us are of nearly the same intensity and saturation.

2. The Wundt colored papers. (E. Zimmermann, 21 Emilienstrasse, Leipzig, Germany.) A series including the various spectral hues and purple in a number of saturations (chromas).

3. The Hering colored papers. (R. Rothe, 16 Liebigstrasse, Leipzig, Germany. C. H. Stoelting Company, Chicago, Ill., American Agent.) A series of twelve strong colors, including purple.

Rothe supplies also a set of fifty neutral papers, ranging from white to black, but it is unsatisfactory.

4. The Bradley colored papers. (Milton Bradley Company, Springfield, Mass.) A useful series, including a great variety of colors and saturations.

Colored cloths.—Cloths, as secondary sources of chromatic stimuli have most of the defects and few, if any, more merits than papers. They are useful, under certain circumstances, for qualitative work.

Especially valuable in this class of reflecting surfaces, because it reflects only a small quantity of white light, is silk velvet. In experiments which demand change in the intensity of the chromatic stimulus without marked change in its hue or saturation this material, properly dyed, is superior to colored papers. A great variety of hues of silk velvet are on the market, but they are quite likely to prove unsatisfactory for tests of color vision because their dyes reflect, in varying amounts, light of different wave-lengths. Could we obtain a set of these cloths which reflected respectively only red, yellow, green, blue, and violet, we should be able to use them to advantage in many of our qualitative experiments on vision in animals. But they would

not meet the fundamental requirements, controllability and describability.

Although for quantitative experiments the reflection method, in its available forms, is quite unsatisfactory, it is not to be discarded lightly, for it offers important conditions for the preliminary investigation of the nature of an animal's color vision which no other method furnishes. Above all, the naturalness of the stimulus, and of the conditions under which it may be applied, would seem to be important. We wish, therefore, to recommend the employment of papers, cloths, and oil pigments under suitable conditions, and with recognition of their limitations. No experimenter can reasonably hope to gain adequate knowledge of the visual capacity of an animal by the use of this method alone, although by means of it he may obtain knowledge which will enable him to formulate his problems and advantageously apply other methods to their solution.

b. The transmission method.—In this rubric we have tried to include all suitable means of obtaining chromatic stimuli by selective absorption and transmission. Among the most important of these means are glasses and gelatines ("dry filters") and solutions ("wet filters"). Before taking up these several types of absorption media, we may enumerate the chief merits and defects of the transmission method, in contrast with the reflection and dispersion methods.

Merits: Availability in many forms; cheapness, in comparison with all forms of apparatus for the dispersion method; convenience and simplicity of apparatus (this applies especially to glasses and gelatines—the so-called dry filters—it does not hold to the same extent of wet filters); the possibility of use in daylight and sunlight; ease of changing quality and intensity of stimulus independently, within certain limits; reasonable ease and accuracy of measuring wave-length and intensity of stimulus. (In the last two features the transmission method is infinitely superior to the reflection method.)

Defects: Inconstancy of qualitative values of commercial colored glasses and gelatines (this does not apply to solutions); more or less rapid fading (this renders unsatisfactory expensive sets of glasses); unnaturalness of the stimulus, as compared with colored papers, or cloths or other surfaces, viewed in sunlight (it is possible to use color filters in sunlight, but their

light is not so satisfactorily applied thus as in a dark-room, and quantitative experiments are impossible); impossibility of obtaining readily any desired quality of stimulus.

Unlike the reflection method, the transmission method may be used fairly satisfactorily for quantitative work, for the stimulus may be accurately measured and it may be applied, in a dark-room, under simple controllable conditions.

Since animals in nature view colors mostly as colored objects seen in white light, it seems only fair that their color vision should be investigated under conditions similar to these, as well as under such conditions as are furnished by the clear cut and controllable methods of transmission and dispersion. The quantitative tests described in this report are open to the objection that the darkened room, in which they should be conducted, is an unnatural environment for most animals. In view of this objection, we believe that for rough qualitative tests of vision the reflection and the transmission methods may be employed to advantage in daylight or sunlight. But even in the case of qualitative experiments in sunlight, we recommend the transmission method over the reflection method because it enables the experimenter, first, to get a great variety of qualities (hues) and intensities; second, because it enables him to measure both the wave-length and intensity; and third, because it enables him, within narrow limits, to vary intensity independently of quality.

We shall now point out the special characteristics of a number of the substances which exhibit selective absorption and transmission.

Glasses.—Colored glasses are readily obtainable, but most of them, when used singly, are practically worthless as filters. There are two kinds in use: that in which the pigment is mixed with the glass, known as pot glass; and the variety which has a thin coating of pigment on one or both surfaces, known as flashed glass. The former in good quality, is more expensive than the latter. To these we might add the spectro-optical glass, a special kind of pot glass

Colored glasses may be described as ray filters in relatively stable and permanent form, convenient for handling. Unfortunately many colored glasses fade more or less rapidly in strong

light. Their chief advantage over solutions for visual tests is portability.

At present the following are the most satisfactory colored glasses of which we have knowledge, together with sources of supply:

E. Grosse, Berlin, N. W. 52, Paulstrasse 5, supplies flashed glasses which yield red, yellow, green, blue, and violet light. These glasses are, on the whole, the most satisfactory of the flashed glasses which we have been able to obtain. Only the red, however, is monochromatic. By using combinations of the blue and violet glasses, it is possible to get a stimulus which contains no red or yellow. If monochromatic light is desired these glasses, with the exception of the ruby, are of no value.

Chance Brothers and Company, Birmingham, England, manufacture colored glasses which are used for signals, but examination indicates that except in combination they have no special value for our purposes.

The Central Scientific Company, 14-28 Michigan street, Chicago, Illinois, sells (a) plates of pot glass, and (b) plates of flashed glass. The latter they furnish in seven colors. Plates 10 cm. square, 11 cents each. These glasses, according to our spectroscopic examinations are not as satisfactory as those supplied by E. Grosse.

Schott and Genn, Jena, Germany, manufacture a high grade of colored glass (spectro-optical), much more expensive and also more satisfactory than the flashed or pot glasses of commerce. Their latest series consists of six colored glasses: "dark, medium, and light yellow, blue filter, green glass, and red filter." These may be obtained in different thicknesses, and, according to catalogue statements, in pieces not exceeding 5 cm. square. The price of pieces approximately 4 cm. × 4 cm. is about $1.25 each.

Glass filters greatly reduce the intensity of a chromatic stimulus and it is therefore necessary to employ with them an intense source of light. They are of extremely different values with respect to different wave-lengths.

For red one or two thicknesses of flashed ruby glass is excellent.

For yellow no colored glass, or combination of glasses, is satisfactory.

For green the same is true. A monochromatic green can not be obtained with any of the glasses, or combinations thereof, which we have examined.

For blue a glass may be obtained which transmits no red, but blue can not be obtained without some violet.

For violet there is no satisfactory glass or combination of glasses.

From these statements it is evident that among the colored glasses at present manufactured, it is extremely difficult to find satisfactory filters for use in the study of color vision. With commercial glasses it is easy to obtain two mutually exclusive chromatic stimuli (red and blue-violet for example), and it is possible to divide the spectrum into three mutually exclusive parts (red, yellow-green, blue-violet), but the search for the proper glasses, and combinations thereof, for the latter purpose is likely to prove discouraging and scarcely worth while, in view of the possibility now to be mentioned of obtaining good gelatine filters.

The ruby glass of Grosse we find perfectly satisfactory as a source of red light, and could equally good glasses be obtained for yellow, green, blue, and violet, our qualitative experiments on color vision would be greatly facilitated.

On the whole, it seems desirable that some one should attempt to discover formulae for a set of colored glasses which shall be as satisfactory as the ruby. The task is a difficult one, and, although we have considered undertaking it, we have not found time nor opportunity to do so.

Gelatines.—Color filters consisting of pigmented films of gelatine yield color stimuli by selective absorption and transmission. There are, so far as we know, no gelatines on the market which, when used singly, give monochromatic stimuli, but by combining certain of the films now available it is possible to obtain fairly satisfactory filters for red, green, yellow, and blue. There is no apparent reason why a standard set of gelatine filters which should yield respectively red, yellow, green, blue, and violet light should not be manufactured. They should be prepared with extreme care, according to definite formulae, by a reliable scientific firm, and the experimenter would need to be on his guard against fading, for one of the chief defects of gelatine films is their extreme liability to fade.

Spectroscopic, as well as spectro-photometric tests would have to be made frequently. But in spite of this, it seems to us desirable that a standard set of gelatine filters be manufactured. Such a set of films would possess almost all of the merits of colored papers, while enabling the experimenter to vary, somewhat, the intensity of his chromatic stimulus independently of its hue and saturation.

Available at present are the following sets of pigmented gelatine films:

Hanauer vereinigten Gelatoid Fabriken, Hanau, a. M. Germany, manufactures colored gelatine films, some of which, singly or in combination, are excellent. They may be obtained through the American Agent of the German firm, Henry Pfaltz, 300 Pearl Street, New York.

Queen and Company, 1211 Arch Street, Philadelphia, imports and sells a set of thirty gelatine absorption films, 3½ by 6½ inches, stained by chemically pure substances. They are too thin for our purposes, and in our tests proved of little value.

Zimmermann, 21 Emilienstrasse, Leipzig, Germany, supplies gelatine films for psychological purposes.

Filters are made, to order, by the Cramer Dry Plate Company, St. Louis, Missouri.

Dr. Louis Bell recommends the following formulae, on the basis of his experience:

For red: (1) Grubler's lichtgrün F. S.; (2) concentrated safranine, in gelatine films.

This double filter transmits pure red of 700 $\mu\mu$ wave-length.

For yellow: (1) Copper chloride (solution) in absolute ethyl-alcohol; (2) yellowish eosine (Berlin Anilinfabrik), in gelatine film.

This combination, solution and gelatine, filter transmits yellow of 560–590 $\mu\mu$.

For green: (1) Acid green (Cassella and Co.); (2) methyl orange III (St. Denis Co.), in gelatine films.

This double filter transmits green of 460–490 $\mu\mu$ (?).

For blue green: Methyl green, in gelatine film.

This transmits light of 450–490 $\mu\mu$.

For blue: (1) Alkali blue (Albany Color Works); (2) yellowish eosine (Berlin Anilinfabrik), in gelatine films.

This double filter transmits from 435 $\mu\mu$ to the ultra-violet.

Directions for making up gelatine or collodion filters from approved formulae may be found in works on photographic methods. [1]

The essential points of method may be stated. Lantern dry plates should be (1) fixed in a solution of hyposulphite of soda, (2) cleared in a ferricyanide and hyposulphite solution, if necessary, (3) washed thoroughly, and (4) immersed in the solution of the desired stain until the proper depth of color has been attained.

For the following practical and more specific directions, the writers are indebted to Mr. Willard Greene.

Select a heavily coated dry plate, fix, clear (if necessary) as above, wash for an hour in running water, and dry. Having prepared the dye solution according to formula, pour it into a tray about the size of the plate to be stained. Place one edge of the dry, or partially dry, plate in the solution and by tipping the tray, and at the same time lowering the plate, cause the solution to flow over it in an even wave. Leave the plate in the dye until it has attained proper depth of color. The time may be accurately determined by trial. Upon removal of the plate from the dye, rinse it in running water. Drain and then wipe lightly in both directions with soft surgical gauze to remove excess of solution. After a few minutes wipe again to remove surface moisture and then place on rack to dry.

When skill has been achieved in staining plates to the proper depth, a filter of more satisfactory quality may be made by coating optical glass with a carefully filtered and clarified solution of gelatine or collodion, and, when dry, immersing in the dye-solution for the proper length of time. Gelatine filters may be protected by binding plates of clear glass against them.

Solutions.—(Wet filters) Aqueous or alcoholic solutions of dyes and other substances exhibit selective absorption (the capacity to transmit only light of certain wave-lengths). The quality and intensity of the light transmitted depends upon the concentration of the solution and the thickness of the layer. Either singly or in series, layers of solutions which exhibit selective absorption may be used as ray filters. The absorption

[1] Bolas, T., Tallent, A. A. K., and Senior, E. A handbook of photography in colors. New York and Chicago, 1900. Chapter 21. "The manufacture of color filters," pp. 157–160.

spectra of hundreds of solutions have been described.[1] We have ourselves examined many substances in our search for *single solutions* which should yield satisfactory monochromatic stimuli, but our efforts have not added materially to the knowledge of the subject.

Single solutions which transmit "monochromatic light" in fair intensity are rare. Indeed, a serious fault common to filters is the great diminution of the intensity of light. Except with a high intensity source it is impossible, in most instances, to obtain a narrow band of light of fair intensity. This defect is gr atly accentuated when two or more layers of solutions (or glasses or gelatine films) are used side by side. Another serious defect of ray filters is the transmission of the infra red and ultra violet.

Wet filters have the advantage of being relatively easy to make. They have almost all of the advantages of dry filters, and their deterioration is a less serious matter because of the ease with which they can be renewed.

In order to insure "keeping" for a considerable interval, solution filters should be made up with distilled water which has been boiled for several minutes before dye is added. The addition of a few crystals of carbolic acid to solution after it has been cooled will prevent the growth of moulds.

For single solution filters, boxes of crystal plate, optical glass, or quartz, in certain standard sizes and shapes (or to order) may be obtained from E. Leybold's Nachfolger, Cöln a Rh., Germany. These cells are admirably suited to the needs of students of color vision who wish to obtain chromatic stimuli by means of wet filters.

For double or triple solution filters the Leybold glass boxes may be obtained on special order, with a single plate of glass between adjacent chambers.

We have designed a special unit cell which has many ad-

---

[1] Kayser, H. Handbuch der Spectroscopie. Leipzig, 1905, Bd. III.

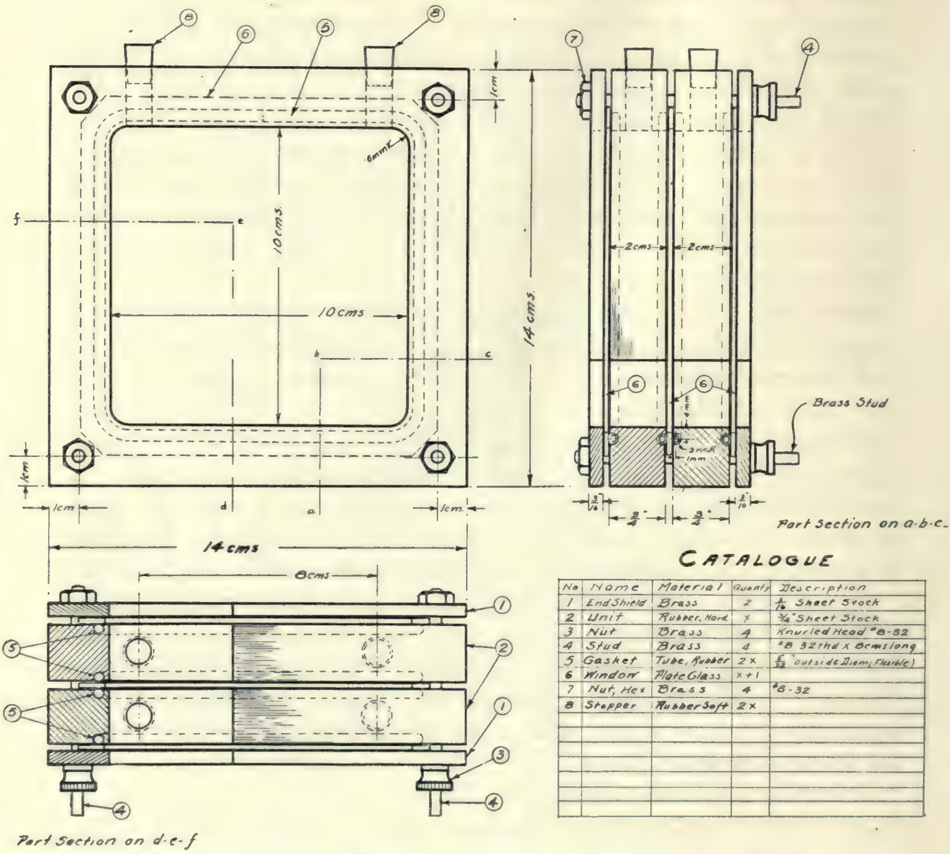
Uhler, H. S. and Wood, R. W. Atlas of absorption spectra. Washington, 1907. Carnegie Institution Publication, No. 71.

Jones, H. C. Hydrates in aqueous solution. Washington, 1907. Carnegie Institution Publications, No. 60.

Formănek, J. Die qualitative spectral analyse anorganischer Körper. Berlin, 1900.

Formănek, J. Spectral analytischer Nachweis kunstlicher organischer Farbenstoffe. Berlin, 1900.

vantages. It may be used for single, double, or triple solutions, and it is convenient to fill, empty, and clean. Any kind or thickness of glass may be used as transmitting plates, for the whole may readily be taken apart.



No	Name	Material	Quantity	Description
1	End Shield	Brass	2	$\frac{1}{16}$ Sheet Stock
2	Unit	Rubber, Hard	x	3/4" Sheet Stock
3	Nut	Brass	4	Knurled Head #8-32
4	Stud	Brass	4	#8 32thd x 8cms long
5	Gasket	Tube, Rubber	2x	$\frac{6}{32}$" outside Diam (Flexible)
6	Window	Plate Glass	x+1	
7	Nut, Hex	Brass	4	#8-32
8	Stopper	Rubber Soft	2x	

FIGURE 5—Unit absorption cell for ray filter. The drawings represent a double (two unit) cell.

In fig. 5 we present construction drawings for a two-unit cell of this type.

The essential parts of the cell are a solution chamber cut in a 14 x 14 x 2 cm. block of hard rubber; two 14 x 14 x .4 cm. plates of sheet brass; two plates of optical glass 12 x 12 cm.; two rubber stoppers; rubber tubing for gasket; four studs

and nuts. The clear aperture of this cell is 10 x 10 cm. In the form used by us, it carries a 2 cm. layer of solution, but it may as readily be made for a 1 cm. layer.

At the outset of this investigation it was our ambition to select a set of wet filters which should yield the principal spectral hues in a satisfactory manner. This we have not succeeded in doing, and we therefore present the following formulae—all of which have proved satisfactory under certain circumstances —for the consideration of those who wish to experiment with wet filters.

The formulae of Landolt.[1]—These we have tested and found airly satisfactory with respect to the quality of the light transmitted. The yellow is unsatisfactory because of the low intensity of the transmitted light.

Color	Thickness of layer in mm.	Aqueous solution	Grams of substance in 100 cc. of solution	Optical point of max. value
Red	20	Crystal violet 5 Bo.	0.005	665.9 $\mu\mu$
	20	Potassium monochromate.	10	
Yellow	20	Nickel sulphate + 7 aq.	30	591.9 $\mu\mu$
	15	Potassium monochromate.	10	
	15	Potassium permanganate.	0.025	
Green	20	Copper chloride + 2 aq.	60	533.0 $\mu\mu$
	20	Potassium monochromate.	10	
Light blue	20	Doppelgrün S, F.	0.02	488.5 $\mu\mu$
	20	Copper sulphate + 5 aq.	15.	
Dark blue	20	Crystal violet 5 BO.	0.005	448.2 $\mu\mu$
	20	Copper sulphate + 5 aq.	15	

[1] H. Landolt. Das optische Drehungsvermögen. 2 Auflage, Braunschweig, 1898, S. 390.

The formulae of Parker and Day.—The following formulae were devised for the study of the reactions of organisms to chromatic stimuli in the Zoölogical Laboratory of Harvard University, but for obvious reasons they were not employed. We present them as given to us by Professor G. H. Parker, with his comments.

Color	Thickness of layer in mm.	Aqueous solution	Grams of substance in 1300 cc. of solution	Range in spectrum
Blue	20	Methylene blue	0.65	420-470 $\mu\mu$
Green	20	Napthol yellow S	3.25	490-580 $\mu\mu$
		Lichtgrün F. S.	.39	
		Napthol green B	.39	
Yellow	20	Napthol yellow S	3.25	560-650 $\mu\mu$
		Napthol green B	.39	
		Ponceau P. R.	.20	
Red	20	Ponceau P. R.	13.00	600-720 $\mu\mu$?
		Napthol green B	.20	
		Formyl violet S 4 B.	.32	

"All of these were used as single solution ray filters in plate glass containers, with the thickness of layer 20 mm. Tests with the radiomicrometer indicated that about one-third of the energy transmitted by these solutions was invisible (probably for the most part infra-red). We could find no satisfactory way to prevent this transmission of the invisible rays and therefore abandoned the use of filters in connection with studies of the influence of chromatic stimuli."

The formulae of Greene.—We have tested the following formulae, supplied by Mr. Willard Greene, and discovered that they may be used to advantage for certain experiments with animals. Several of them are very similar to those given by Professor Parker. We have used our solutions, however, in layers 10 mm. instead of 20 mm. in thickness.

Color	Thickness of layer in mm.	Aqueous solution	Grams of substance in 1 liter. of solution	Range in spectrum
Violet	10	Copper sulphate (c. p.)	25.00	–450 $\mu\mu$
		Ammonia	10.00 c.c.	
Blue	10	Napthol green B[1]	.20	450–490 $\mu\mu$
		Formyl violet S. 4B.	.20	
Green	10	Napthol yellow S.	2.50	510–550 $\mu\mu$
		Lichtgrün F. S.	.30	
		Napthol green B.	.30	
Yellow	10	Napthol green B.	.30	560–610 $\mu\mu$
		Napthol yellow S.	2.50	
		Ponceau P. R.	.15	
Red	10	Ponceau P. R.	10.00	600–700 $\mu\mu$

Lithium carmine, in aqueous solution, yields a fairly satisfactory red.

R. W. Wood in his "Physical optics" (New York, 1905, p. 12), recommends the following methods of obtaining monochromatic light from the mercury arc.

He writes, "for long continued work, however, the most satisfactory light is the mercury arc,[2] from the radiation of which we can pick out by means of color screens . . . any one of the numerous bright lines. The following screens have been recommended for use with this form of lamp. The solutions are made with water, and contained in cells made by cementing glass plates to annular strips cut from heavy brass tubing.

Methyl violet 4 R (Berlin aniline fabrik) + very dilute nitrosodimethyl-aniline, transits wave-length 365. Methyl violet + chinin sulphate (separate solutions), the violet solution is made strong enough to blot out wave-length 4359. This screen transmits 4047 and 4078, also faintly 3984.

Cobalt glass + Aesculin solution, transmits 4359.

Guinea green B extra (Berlin) + Chinin sulphate, transmits 4916.

Neptune green (Bayer, Elberfeld) + Chrysoidine. Dilute

[1] Dyes from H. A. Metz and Company, New York.

[2] Mercury lamps, made of fused quartz, may be obtained from W. C. Heraeus, Hanau, Germany.

the Chrysoidine sufficiently to just transmit 5790 and 5461, then add Neptune green until the yellow lines disappear.

Chrysoidine + Eosine transmits 5790. The chrysoidine should be dilute and the eosine added until the green line disappears."

For additional formulae and a thorough discussion of ray filters the reader is referred to the recent monograph of Nagel[1] and to Busck.[2]

Except when a light which gives a line spectrum is used for special purposes, as for example in the case of the mercury lamp, it is desirable to use as a source in work with the transmission method a white light of fairly high intensity. Sunlight, daylight, Nernst light, tungsten light, and acetylene light are likely to prove satisfactory. The sun is to be preferred as a source when naturalness is the chief desideratum; the Nernst or tungsten lamp, when constancy of intensity is important. The transmission method of obtaining chromatic stimuli is decidedly superior to the reflection method in that it permits (a) of the use of a greater variety of stimuli, (b) of the control of the quality and intensity of the stimuli to a greater extent, (c) of more accurate determinations of the wave-length used, and finally, (d) of the spectrophotometric measurement of intensity in a fairly satisfactory manner. On account of the unequal transmission of the infra-red rays by filters, radiometric measurements are of little value.

Dry filters are convenient for a great variety of experiments which demand neither stimuli of a single or closely restricted wave-length, nor extreme accuracy of measurement.

c. Dispersion method.—White light may, by various means, be resolved into a spectrum,[3] from which the experimenter may select and isolate light of the particular wave-length he desires to use as a stimulus.

We may mention, as especially convenient for use in studies of color vision, two mechanisms for the production of spectra: the diffraction grating and the prism.

Grating spectra are excellent for many of the investigations

[1] Nagel, W. A. Methoden zur Erforschung des Licht und Farbensinns. Tigerstedt's Handbuch der physiologischen Methodik. S. 43–55.
Nagel, W. A. Ueber flussige Strahlenfilter. Biologisches Centralblatt, 1898, Bd. 18, S. 655.

[2] Busck, G. Ueber farbige Lichtfilter. Zeitschr. f. Psychologie und Physiol. d. Sinnesorgane, 1904, Bd. 37, S. 104–111.

[3] Baly, E. C. C. Spectroscopy. London, 1905.

of human vision, but because of their low intensity, in comparison with prismatic spectra, they are less satisfactory than the latter where a wide range of intensities for a given hue is demanded. As soon, however, as it becomes possible to rule a large concave grating with short focus, the diffraction grating, in all probability, will supersede the prism, since with such a grating collimating and objective lenses would be unnecessary. The grating would have the additional advantage of rendering it possible to get a good yellow. This is extremely difficult with the prism.

Prismatic spectra seem to us, at present, the most satisfactory chromatic stimuli. We have therefore decided to use them in connection with our standardized apparatus for the investigation of color vision in animals.

The advantages which spectral stimuli obtained by "diffraction" or "refraction" have over chromatic stimuli obtained by "reflection" or by "transmission" (as used in this report) may be thus stated:

(1) They are perfectly under the control of the experimenter with respect to (a) color, (b) saturation, and (c) intensity.

(2) They are measurable, and therefore describable, with a degree of facility and accuracy which is not attainable in connection with other methods.

Inasmuch as the remainder of this report is to be devoted to the description of a method of using spectral light, it is needless to discuss the matter further at present.

3. Measurements of stimulus

Whatever the means employed for obtaining chromatic stimuli, they should be accurately describable in terms of wave-length (quality or composition) and intensity (psycho-physiological and physical).

As has been pointed out, it is practically impossible to describe stimuli obtained from surfaces which exhibit selective absorption and reflection. It is considerably less difficult, however, to describe stimuli obtained by the use of ray filters. Finally, it is possible to obtain highly satisfactory descriptions of the stimuli used in the "color" apparatus now to be described.

We recommend that in all quantitative investigations of color vision determinations be made (a) of the wave-length of the stimuli by means of a first-class spectroscope or spectrometer,[1] and (b) of the intensity of the stimuli by means of a good spectro-photometer and also, when grating or prism is used, by the use of a radio-micrometer or a selenium cell.[2]

V. DESCRIPTION OF STANDARDIZED APPARATUS AND METHOD FOR THE STUDY OF COLOR PERCEPTION

The apparatus we have finally decided to recommend as a standard for testing the color perception of animals was described in an early form in the April, 1909, number of the Journal of Comparative Neurology and Psychology, p. 1. In that form there were many objectionable features, such as the use of silvered mirrors, the projection of colored beams upon improperly ground glass surfaces, etc., all of which have been entirely eliminated in the present form.[3]

In its improved form the apparatus now affords:

(1) Means of selecting any two desired bands of homogeneous spectral light of constant and known wave-lengths. Furthermore, (a) the energy of each band is known in C. G. S. units and can be varied at will; (b) the distance between them is adjustable; (c) their right-left position can be interchanged at will.

(2) Means of splitting any single chosen beam of monochromatic light into two parts, in such a way that the two beams thus obtained are of the same wave-length, form and size, and are equal in energy. At the same time, (a), (b) and (c) under (1) apply here also.

(3) The possibility of substituting for either of the monochromatic bands a beam of white light of the same size and form as the homogeneous chromatic bands.

(4) The alteration of the saturation of any monochromatic band by the admixture of white light of known energy.

---

[1] Further details are to be found on pp. 79–84, in the description of our standard procedure.

[2] See p. 79.

[3] On pp. 70–87 will be found a detailed description of the separate parts referred to here, together with a statement of their cost and the places where they may be obtained.

1. General description of the optical system

Fig. 6 is a diagram showing the path of the light. As it is drawn, the arc L is used as a source. The apparatus is adaptable for use also with sunlight or with a Nernst filament. When sunlight is used, the beam from a heliostat falls directly upon the condenser, K. When the Nernst filament is used it is mounted vertically 12 cm. from the slit S, and is focused upon S by means of a small concavo-convex lens of $\frac{3}{8}'$ aperture and $1\frac{1}{2}''$ focus (single achromat). Such lenses are ground to order for us by the Wollensak Optical Company, Rochester, N. Y.

It will be seen as the apparatus is here sketched for use with the arc, that the light from the crater of the positive carbon of the arc L falls upon the face of an achromatic condenser, K. This condenser causes a sharp image of the crater of the positive carbon to fall upon the slit S. The diverging rays of light issuing from this slit are made parallel by the collimating lens C. The parallel cylinder of light next strikes the face of a dense flint glass prism (60°), is refracted, and passing through the objective O, is brought to a focus in a series of colored images of S upon the double slit (at R and G, two beams are shown passing through this slit).

Immediately behind this double slit two total reflection prisms m_1 and m_2 are to be found. As shown in the diagram, they serve the purpose of bringing the red beam near to the green. It is absolutely necessary to keep the two selected beams within about 10 mm. of each other if the reversing and spacing devices which are later described are to work properly. It may be seen that the distance separating the two beams shortly after they issue from the double slit is determined by the separation existing between m_1 and m_2. Suppose, for example, one beam issues from the red region, and the other from the violet. Now, with the lenses and refracting prism at present in use, the separation would be, without the use of m_1 and m_2 about 6 cm. In order to bring the red over to the violet and have them in the same relation as are the red and green in the diagram, one has to draw prism m_2 over toward the violet beam until the red issues from m_2 parallel to the violet and 10 mm. distant from it. This device works equally well for any two selected beams widely separated. On the other hand, if the

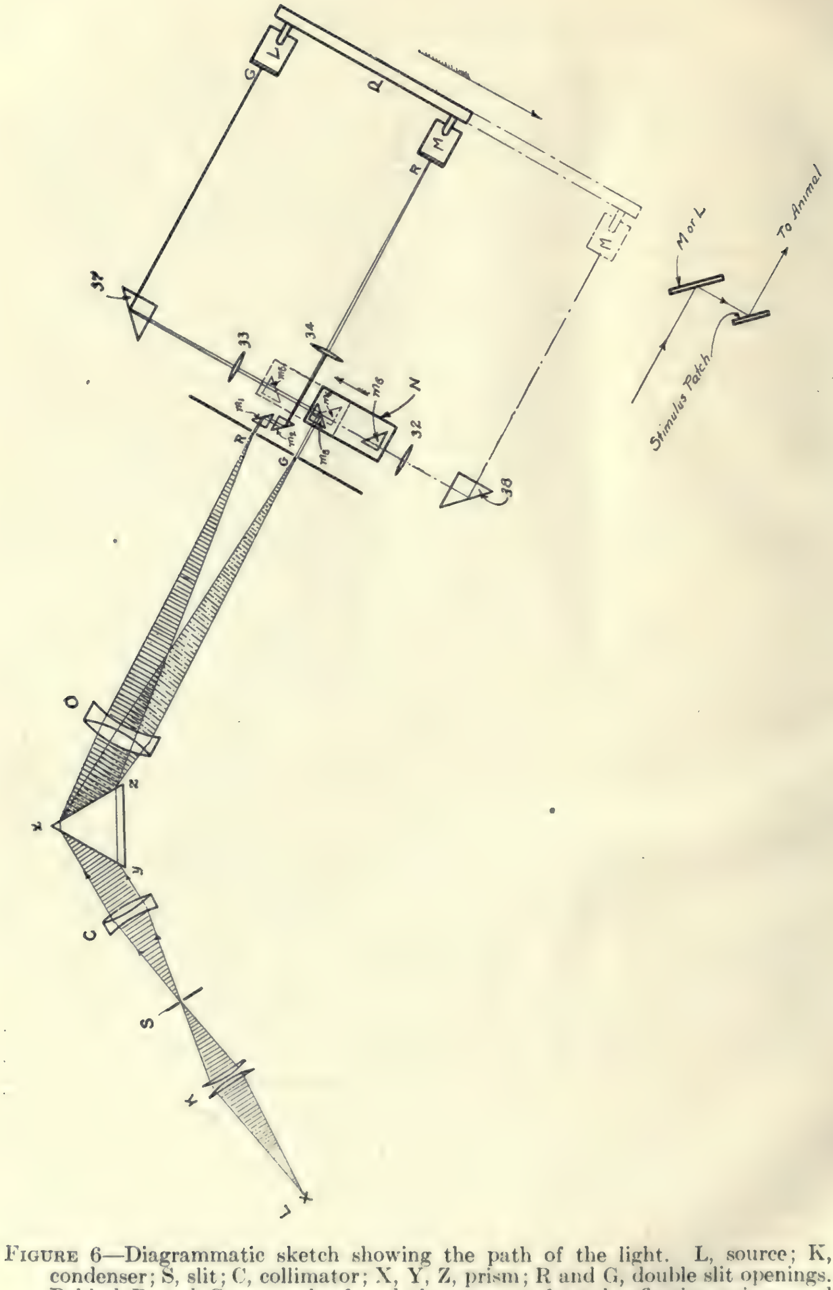


FIGURE 6—Diagrammatic sketch showing the path of the light. L, source; K, condenser; S, slit; C, collimator; X, Y, Z, prism; R and G, double slit openings. Behind R and G are to be found the system of total reflection prisms and small lenses used in spacing, reversing and projecting the monochromatic bundles. M and L are speculum mirrors for receiving the projected beams. The small diagram shows the path of the beams after reflection from the mirror.

beams lie very near together, as e. g., red and orange, the prisms m_1 and m_2 are unnecessary. Beams from regions lying very near together, e. g., two in the red, can still be spaced and reversed within the limits of the construction of the double slit (cf. p.62).

At times it becomes necessary to have two beams of exactly the same wave-length, for example, when one desires to test the difference limen for any given color (D. L.). The apparatus permits this with only a slight modification (not shown in the above diagram, but separately below—fig. 6A).

One allows a single beam to issue from the double slit and to fall upon a Wollaston double image prism, combined with two parallelopipeds of glass, the arrangement of which is shown in the figure. A, slit admitting the monochromatic bundle, B;

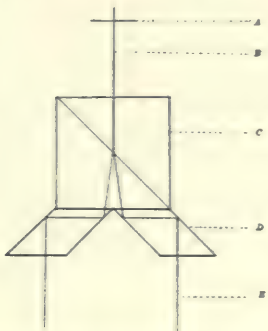


FIGURE 6A—Apparatus for obtaining two beams of same wave-length.

C, Wollaston double image prism; D, one of the parallelopipeds of optical glass (equivalent to two total reflection prisms); E, one of the divisions of the bundle, B. This compound prism (designed especially for our work by Steeg & Reuter, Homburg, v. d. Höhe) yields us two parallel beams of light, 10 mm. between centers, of exactly the same wave-length and of equal energy. They can be spaced and reversed as are the red and green in fig. 6.

As may be seen from the solid line marked G, fig. 6, the green beam falls upon the face of the total reflection prism, m_5, passes through the small achromatic lens 33, strikes the total reflection prism, 37, whence it is reflected at right angles to fall upon the speculum mirror, L. It is reflected downward at right angles by this and made to fall upon a plaster of paris strip below,

as is shown in the small auxiliary diagram. We thus have falling upon the plaster of paris surface a sharply focussed and magnified image of slit G. This image is always larger than the plaster of paris strip. The excess light passes down into a dark compartment and is absorbed.

The red beam, after passing through the small right angle prisms in the way already described, escapes behind the base of the prism m_5, passes through the small projection lens 34, thence to the speculum plate M, and down to the plaster of paris surface, as in the case of the green. As conditions are in the diagram, the red beam is on the left, the green on the right.

In order to reverse the position of the two colors, the slide N, bearing the prisms m_5 and m_6 must be pushed over to the right to take the dotted position. The green beam now engages the right angle prism m_6, is bent at right angles through lens 32 to meet prism 38, thence to a mirror and plaster of paris surface as before. After this change is made, the red beam is made to pass between m_5 and m_6. Its course is in no way changed by the shifting of the slide N; as conditions are now after the introduction of this change, the green beam is on the left and the red on the right. If only two speculum mirrors are used, the carrier P, holding them must be pushed over to the left to take the dotted position. If three speculum plates are used, no shifting of them is required (cf. p. 56). It is hardly necessary to mention that m_5 and m_6 are prisms of the same size and absorption value; that prisms 37 and 38 are likewise matched, and that lenses 32 and 33 are matched as to quality of glass, thickness, and focal length.

There are three convenient ways in which the energy of these beams may be altered: (a) by attaching iris diaphragms to the lenses; (b) by the interposition of a smoked wedge in the pathway of each beam, and (c) by the rotating sector or episcotister. The rotating sector is the easiest and most accurate method of the three. Figs. 7 and 11 show the arrangement for use with the episcotister cf. also p. 76).

The method by which a white beam of light equal in size and form to the monochromatic beam is made to fall upon the plaster of paris strip in place of either of the monochromatic beams is as follows: A Nernst filament is mounted vertically

No. 17.
page 50A.

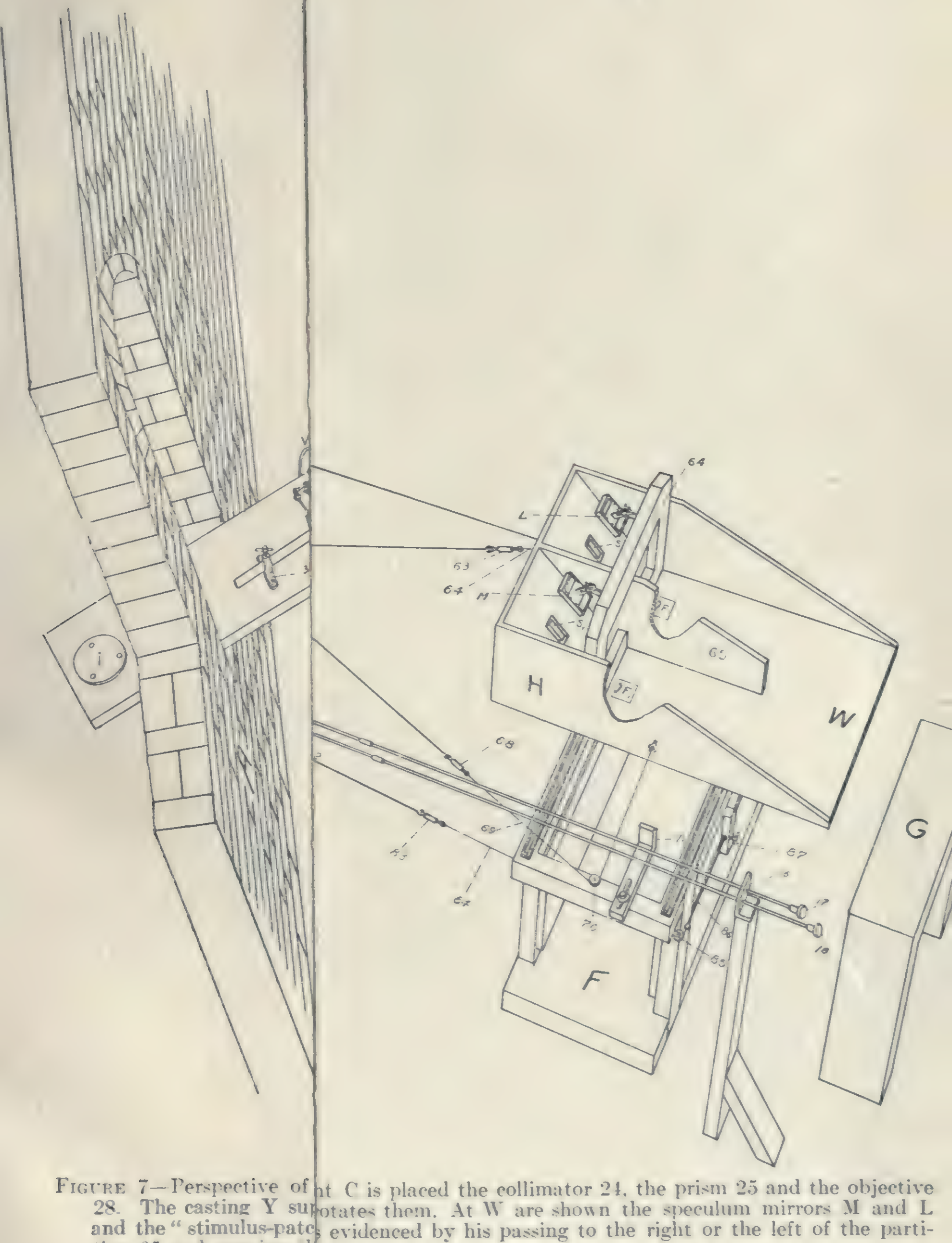


FIGURE 7—Perspective of t C is placed the collimator 24, the prism 25 and the objective 28. The casting Y su otates them. At W are shown the speculum mirrors M and L and the "stimulus-patc evidenced by his passing to the right or the left of the partition 65 and opening th

in a 4″ cubical metal box. (At greater expense the Westinghouse Nernst lamp, see p. 5, may be mounted on the box in place of the filament. The lamp is far more convenient.) This filament is projected upon the plaster of paris strips by means of a short focus achromatic lens mounted in a tube attached to the box. The box can be shifted on a metal arm so as to exclude any one of the beams, and to project in its place a white beam upon the plaster of paris surface. This box is not shown in the diagram. It should be inserted immediately behind the partition D, shown in fig. 7, and consequently in front of the episcotisters. The episcotisters can then be adjusted so as to decrease the energy of this light to any desired amount. If preferred, a simple vertical optical slit may be mounted in the focus of the projection lens. The intensity of the beam may be conveniently and accurately controlled by the slit without the use of the episcotister.

The details of the mounting of the above optical system will now be taken up with the aid of separate drawings.

2. Description of the mounting of the apparatus

Fig. 7 shows the complete system ready for operation. Since the aim of our report is to present the working parts clearly enough to make duplication possible, the description which follows will be somewhat technical and detailed. Also there will be, necessarily, a certain amount of repetition in the descriptions, since fig. 7, showing the whole apparatus, is first taken up, and is followed by the description of the larger drawings of certain parts of the system. Figs. 6 and 7 will give a clear idea of the whole apparatus. Figs. 8-12 inclusive are given for the benefit of those who may desire to construct, or at least install the apparatus. Many important dimensions are not given, but the lenses and other optical parts obtainable differ so much that it seemed hardly worth while to give all the dimensions. Those which relate specifically to the control of the stimulus are everywhere given.

As shown in fig. 7, two rooms, *O* and *I* are devoted to the apparatus. *I* is a complete dark-room 25 feet long and 16 feet wide; *O* is a smaller room containing a window for the admission of sunlight. When either the arc or Nernst is in use, daylight is excluded from *O*. The dividing wall between *O* and *I* is marked

by the letter D. The wall A is the outer wall of the building. It faces west (south is a far better exposure if it can be obtained). With a Fuess heliostat, sunlight may be obtained from about 1 P. M. to 4 P. M.

A shelf, B, 10″ wide and 2″ thick, supported upon heavy trestle work 20″ in height, projects through a window in the wall A. Continuous with this, at an angle of 120 degrees to it and in the same plane, is a much wider shelf, C. This latter shelf, about 30″ wide and 4′ long, is surrounded by walls 2″ thick and 12″ high. The compartment thus formed is supplied with a heavy wooden cover not shown in the diagram. Its shape is somewhat irregular, as appears in the figure. Most of the delicate parts of the apparatus are assembled here. It is lined with dark paper and is made light-tight and dust-proof. Where possible, it is very desirable to have the supports for the parts of the apparatus thus far described made of concrete.

Upon the end of shelf B, which projects through the wall A, is to be found a circular aluminum plate, 1, to hold the heliostat, when sunlight is desired. Two end supports, 3 and 4, screwed to the shelf, support a steel bar, 2. To this rod is attached an arc mechanism, 5, and a clamp, 6, which carries the prism table, 7, shown inside box *C*. Two castings, 8 and 9, carry $\frac{1}{4}''$ rods, 10, 11, 12 and 13, which are so geared that the arc may be controlled by the operator in the room *I*, by turning the knobs 17 and 18. These long rods are supported by means of castings, 14, 15 and 16.[1]

The incident beam (sunlight or arc) is made to pass through the condensing lens 19. This is a "second-hand" Perry portrait lens, 3″ aperture, 6″ focus, supplied with rack and pinion. It gives an image of the crater of the positive carbon upon slit 20. The width of the slit is controlled by the micrometer screw 21. The beam of light may be kept from passing through the slit when desirable by means of a little shutter, 22, electromagnetically actuated. The shutter can be operated by means of a switch placed near the operator at G.

After the rays pass through the slit they are made parallel by means of the collimating lens 24 (Voitländer portrait lens

[1] While the ordinary 45° hand feed arc supplied with the controlling rods just described is satisfactory and cheap, the automatic lamp of Bausch & Lomb is much more convenient. When it is used no controlling rods, castings, etc., are necessary. (See p. 71.)

8″ focus, 3″ aperture, supplied with rack and pinion). The parallel rays then strike the face of a dense flint glass prism, 25, (face 4″ x 4″). The excess white light reflected from the face is thrown into the small dark box, 26.

The refracted beam passes through the objective, 28, (Zeiss "extra rapid" focus portrait lens, 3″ aperture, 20″ focus). This lens is not regularly supplied with rack and pinion, and is therefore mounted on a carriage, 29. This objective brings the beam to a focus on the silvered face of a vertically placed slit wall or table, 30. The mechanism of this is taken up separately in figs. 9 and 10. The wall carries four sliding jaws, which form two slits whose widths are adjusted by means of the micrometer screw-head, 31, and by one not shown in the cut. The casting, Y, which holds the slit-wall, also carries a metal track, 35. A metal support (see 22 of fig. 9) which may be fixed to the metal track 35 at any desired point, bears a sliding mechanism, 36, (N of fig. 6) for reversing the beams. The place of this reversing mechanism on track 35 varies slightly, depending on the monochromatic beams selected, but is shown in the cut as occupying the middle of the track. On each side of this is mounted a small projection lens, 32 and 33; near the ends of the track are mounted the prisms 37 and 38. A third projection lens, 34, is mounted upon a metal track projecting at right angles to the main track. (These numbers refer to the same objects as in fig. 6.) Two small prisms lying between lenses 32 and 33 are not clearly shown in this cut, but appear separately in fig. 9. The whole system mounted on Y, which selects, spaces and reverses the beams, will be made clear by reference to fig. 9.

Three small windows, G, R and G_1, 2″ x 2″ each, are made in the wall D, 25 cm. between centers. Only two of these windows are in use at any one position of the colors.

In room *I*, a stimulus carriage, H, is mounted upon the trestlework F, so as to travel right and left. The rectangular wooden frame 64 carries the two 3″ x 5″ speculum mirrors, L and M, inclined at an angle of 45 degrees to the two incident beams. These mirrors are attached to the framework at a height of 14″ from the floor, W, of the stimulus carriage. The two mirrors reflect the beams vertically downward to the rectangular plaster of paris surfaces, S and S_1. F and F_1 show the two food boxes. A partition, 65, separates the two food compartments. In

front of the stimulus carriage, H, is placed the bench G, upon which the experimenter sits.

Between wall D and the stimulus carriage, H, is shown the apparatus for the control of intensity, mounted upon the trestle-work E. A heavy maple block, 46, 2″ x 12″ x 18″, is mounted upon four ball-bearing rollers, 47, so as to slide on track 48 parallel to and synchronously with the carriage H. A small metal spur, 49, is fastened to block 46. By means of this metal spur and the gut cord system 50, which is attached to the metal framework of the reversing mechanism 36, block 46 is made to operate the device for reversing the colors. The gut cord 50 passes around pulley 51, over and down 52, under 53 and under another one not shown in the cut, up and over 54, thence to 55 and back to the opposite side of the framework of the reversing mechanism. Two stops, 56 and 57, are attached to the cord in such a position that the spur 49 can throw the mechanism 36 to the right or left, depending upon the extreme right or left position of block 46. Since it has already been pointed out that this block moves synchronously with the stimulus carriage H it follows that a movement of H to the extreme right or left position serves to reverse the right-left relation of the two beams.

The block 46 is made to move synchronously with the stimulus carriage H by means of a heavy gut cord, 60, which passes over a pulley 61, connects with a small 3-32″ iron rod, 62, through turn-buckle, 63, with gut cord, 64, which runs over a fixed pulley (not shown) fastened to the framework supporting the track upon which carriage H runs, and is attached to the right-hand side of the carriage. With this single cord system in operation it will be seen that when carriage H is moved to the left block 46 is moved an equal distance to the left also. To the opposite end of the block 46 is fastened a similar heavy gut cord, 66, running over a fixed pulley not shown, but similar to pulley 61 on the opposite side, and connecting with the iron rod 67, through turn-buckle 68, with gut cord 69, over fixed pulley 70 to carriage H. It will be seen that this latter link system will not be operative when the carriage moves from the left to the right position, but that it does operate when the carriage is moved from the right to the left.

The extreme positions of carriage H are adjustable by means of a stop, 71, on the left-hand side, and by a similar stop on the right-hand side, which is hidden by the carriage. The extreme positions of block 46 are likewise limited by stops 72 and 73. When carriage H hits its left-hand stop, 71, block 46 hits its left-hand stop, 72. A similar condition obtains for the right-hand position of both.

In the center of block 46 is mounted a circular table, 74, free to rotate in a horizontal plane. On a diameter of this table and 4″ above it is mounted by means of two L-shaped blocks, 75, a metal track, 76, which carries the two episcotisters, P and K. The distance between centers of these episcotisters can be adjusted by clamping them at any desired position on the track. They are belted by means of small round leather belts or rubber bands to a small motor, 77, screwed firmly to the circular table 74. The speed of the motor can be controlled by the rheostat, 78, which is also screwed to the table. The wiring to the motor and rheostat is carried to the table through a ½″ brass tube, 79, which serves to keep the wires from interfering with the beams when the horizontal table is being revolved in order to change the relation of the episcotisters. The rotation of the circular table, and thus of the episcotisters, can be controlled by the operator at G, by means of a continuous cord system passing around a circular drum lying between block 46 and table 74, to which it is concentrically attached (not shown in this cut, but separately in figs. 11 and 12.) The cord system may be traced as follows: The gut cord 80 passes over a pulley, 81, to iron rod 82, to turn-buckle 83, to gut cord 84, to pulley 85, to iron rod 86, through a clamp 87, to another gut cord which runs over a pulley to a turn-buckle not shown, to iron rod 88, to gut cord 87, over pulley not shown, back to the drum.

a. Operation of the system—The purpose of these various pulley systems is to make the shifting of the colors and the control of their intensity absolutely automatic. The operator at G can reverse the colors by merely pushing the carriage to the right or left for a distance of 25 cm. At the same time, this movement of the carriage can be made to control the positions of the episcotisters. With the carriage at its extreme right position the green is on the right and the red on the left.

Episcotister K intercepts the green beam, while P intercepts the red. Shifting the carriage 25 cm. to the left, brings the green beam to the left and the red to the right, but episcotister P now intercepts the green, while K intercepts the red. Sometimes this is desirable. But suppose we desire to shift the position of the color without changing the relation of the episcotisters; one needs then merely to clamp the cord system controlling the circular table at 87. This will cause the circular table carrying the episcotisters to rotate 180° while the carriage H goes from one extreme position to the other. P will thus be made always to intercept the red, and K the green beam. It is desirable at still other times to use the full intensity of the beam. This is accomplished by first rotating the episcotister table 90° from the position shown in the drawing, and by releasing the clamp screw at 87. The episcotisters will no longer intercept either of the beams in either position of carriage H (see, for mechanism of episcotister table, p 66).

A somewhat more expensive but more satisfactory substitute for carriage H is a rectangular dark cabinet, fig. 8, 40″ high, 20″ thick by 40″ wide. The vertical framework of the cabinet is made of four pieces of oak, 3″ x 3″ x 40″, bolted together by heavy iron straps ¼″ thick, 3″ wide and 40″ long. The sides and top are covered with thin wall-board, coated with dead black paint. The inside of the cabinet is lined with dead black velvet, loosely glued to the sides. The three speculum mirrors, M, M_1 and M_2, are fastened by means of their brass framework to the strap Ba, 25 cm. between centers. The mirrors are inclined as before, at an angle of 45° to the incident beam. Below each mirror stands a metal box containing at its upper end the surface of opal glass or plaster of paris, upon which the beam coming from the mirror is to fall. The beams are admitted through the three windows, W, W_1 and W_2. The arrows show the course of the beams, G, Rd and G_1.

There are two sets of these metal boxes: one set of three, which permits the use of a plaster of paris surface, and another of three, of opal glass. For convenience we shall call these respectively "plaster of paris boxes" and "saturation boxes." The plaster of paris boxes are used in all cases where it is desired to give a stimulation of monochromatic light at maximum

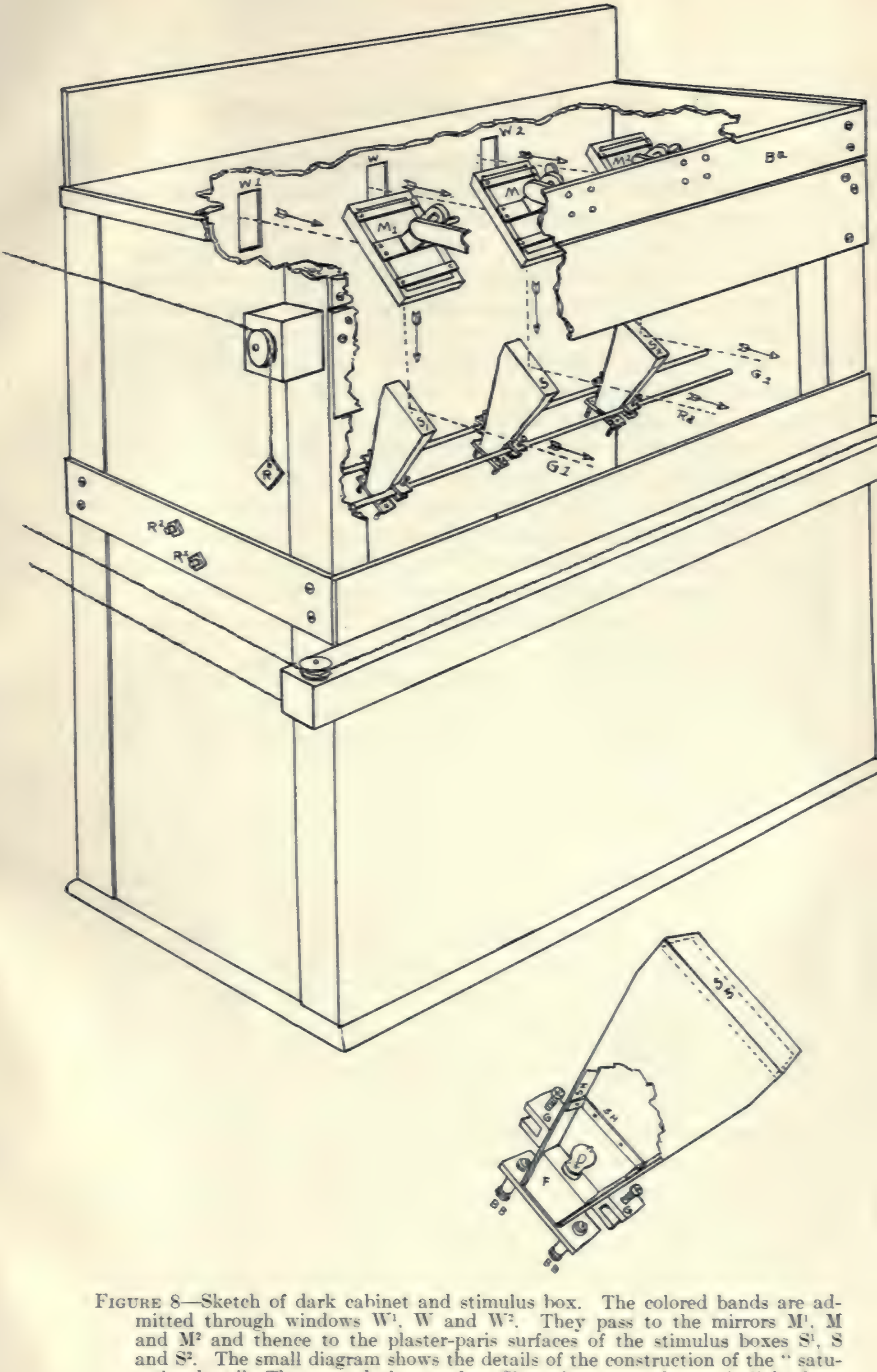


FIGURE 8—Sketch of dark cabinet and stimulus box. The colored bands are admitted through windows W^1, W and W^2. They pass to the mirrors M^1, M and M^2 and thence to the plaster-paris surfaces of the stimulus boxes S^1, S and S^2. The small diagram shows the details of the construction of the " saturation box." The ground glass surface S^3 receives monochromatic light from the speculum mirror above and white light from the tungsten miniature F, below.

saturation. The saturation boxes are employed when it is desired for purposes of control, to alter the saturation of the beam.

The above diagram shows the three saturation boxes in the dark cabinet in the position in which they are to be used. A separate drawing of the interior of the saturation box is shown below the cabinet. The boxes are first cast in brass; the walls are then shaped down until they are made as thin as possible, especially at the large end, which is practically knife-edged. This end is made rectangular, 17 mm. x 7 cm. A piece of opal glass, first ground with coarse emery upon the "flashed" surface (see p. 26) is then accurately ground by hand to fit the whole opening snugly (see dotted lines in small figure). It is then forced in (with flashed surface out) after the edges have been carefully blackened with dead black paint and allowed to dry. This gives a sharply defined surface for the beam coming from the speculum mirror to fall upon. On account of the shape of the box the excess light falls into the dark cabinet and is there absorbed.

The interior construction of this saturation box is clear from the drawing. A small block of wood-fiber, F, is fitted into the small end. A miniature lamp-socket is imbedded in this block, and the wires for its electrical connections are led outside to two binding posts, VB, BB; a small miniature tungsten lamp 1½″ c.p., 2 volts, is burned in this socket. This lamp should have practically a straight filament and should be burned constantly in one position. The tip of the bulb should be at the side and not at the end. The three lamps required are supplied with current by five 2 volt storage batteries. A separate sliding resistance is burned in series with each lamp. A volt meter reading 0–6 volts should also be placed in the circuit, in such a way that the voltage of each lamp may be tested separately. The lamp is burned always at a constant and known voltage. Immediately above the lamp a small shelf-support, SH, 2 mm. wide and 1-64″ thick, is shown; shelves of smoked glass are accurately ground and fitted into the opening. The intensity of the light can be easily and accurately controlled by this means. At a greater expense an Aubert diaphragm, adjustable from the outside of the box, may be inserted in place of the smoked glass.

The opal glass, S_3, is illuminated from below by this white

light, and is at the same time illuminated from above by the beam of monochromatic light coming from the speculum mirror. The energy of the whole effective stimulus may be taken, or that of each component separately.

The plaster of paris boxes are identical with the former in size and shape. The whole upper end of the box, however, is filled with plaster of paris, in accordance with the following method: A good piece of plate glass is first selected. This is then ground with emery of coarse to medium fineness until the surface of the glass shows a uniform grinding. The large end of the metal box is inverted upon this. Dental plaster of paris, mixed with water until it begins to set, is poured into the box and allowed to stand 24 hours. The box is then carefully lifted from the surface of the plate glass, and all excess plaster of paris is removed with a damp cloth, great care being taken not to mar the diffusing surface. After the plaster is thoroughly dry it offers a splendid diffusing surface with a *minimum* of direct reflection. A small dust-proof cap should be kept over these surfaces when the apparatus is not in use.

The stimulus boxes have small metal guides, D and G, attached to their base, supplied with clamp-screws, by means of which the boxes are easily attached to the supporting rods, R^1 and R^2, passing through the cabinet. From the drawing it is clear that these boxes are inclined at an angle of 45°, so that the face of the diffusing surface is parallel to the face of the mirror. Thus, each part of the monochromatic beam passes through the same distance.

If one contrasts the use made of this cabinet with that of the stimulus carriage H, one sees that in the latter there are only two mirrors and two stimulus boxes, and that consequently the whole has to move from left to right, or *vice versa*, in order to catch the green beam as it is sent to the left or to the right by means of the reversing device. The cabinet, however, supplied with three mirrors and three stimulus boxes, remains stationary while the beams are reversed as before, either automatically by the movement of block 46 or by means of a light cord, R, directly connected with the reversing mechanism.

When the cabinet is used the animal is placed in a separate experiment box in front of it (see fig. 4 and p. 84). The cage containing the animal must be movable, parallel to the front

of the cabinet for a distance of 25 cm. Probably the chief advantage of the cabinet comes from the fact that with it experiments may be carried out *in full daylight illumination.*

3. Description of color-spacing and reversing device

The various parts by means of which the two beams are selected, spaced, reversed and finally projected upon the plaster of paris surface, are all assembled upon the iron casting Y, fig. 9. This consists of a base $\frac{1}{2}''$ thick and $3''$ wide, mounted upon three levelling screws, two of which are seen in 1 and 2, and two uprights, 3 and 4. The upright pieces are notched on the back so as to support the metal track 5. This track consists of two $\frac{1}{2}''$ square iron bars, 60 cm. long, bolted together parallel by straps, 6 and 7. The track is held to the uprights by means of clamps 8 and 9. It will be seen that the track can be clamped to the casting in any desired position to the right or left. In the central point in the length of this track, there is attached another short track, 10, normal to it. The front upper part of each of the uprights 3 and 4 is notched so as to receive the slit-wall 11, containing the double slit (see fig. 10). The front plate of this slit-wall is fastened to a rectangular frame, 12, made of $\frac{1}{4}''$ square brass rods. To the upper face of the lower horizontal bar of this frame is attached a grooved track, 13, the beveled sides of which are fastened to a horizontal plate by means of screws, two of which appear at 14 and 15.

Running in this grooved track are to be found two small prism tables, 16 and 17, which can be moved by hand and clamped in any desired position by arrangements not shown in the cut. To these tables are attached two total reflection prisms with 9 mm. face, 18 and 19 (m_1 and m_2 of fig. 6). The use to which these prisms are put is mentioned on page 47. The plate and frame holding the double slit mechanism is shown in front view in fig. 10.

In the present drawing is seen the back view of this double slit mechanism. The letters O, J, J_1, J_2, J_3, have the same significance as in fig. 10. In the back of the slit-wall is the window W, the slit jaws, J, J_1, J_2, J_3, slide in front of this. Jaws J_1 and J_2 are held firmly against the track by means of spring clips, c and d. The two slit openings described in fig. 10 are

seen here from the back at Sg and Sr. These two slits admit the two selected beams, e.g., red and green.

The track 5 supports five 1″ square iron posts, 20, 21, 22,

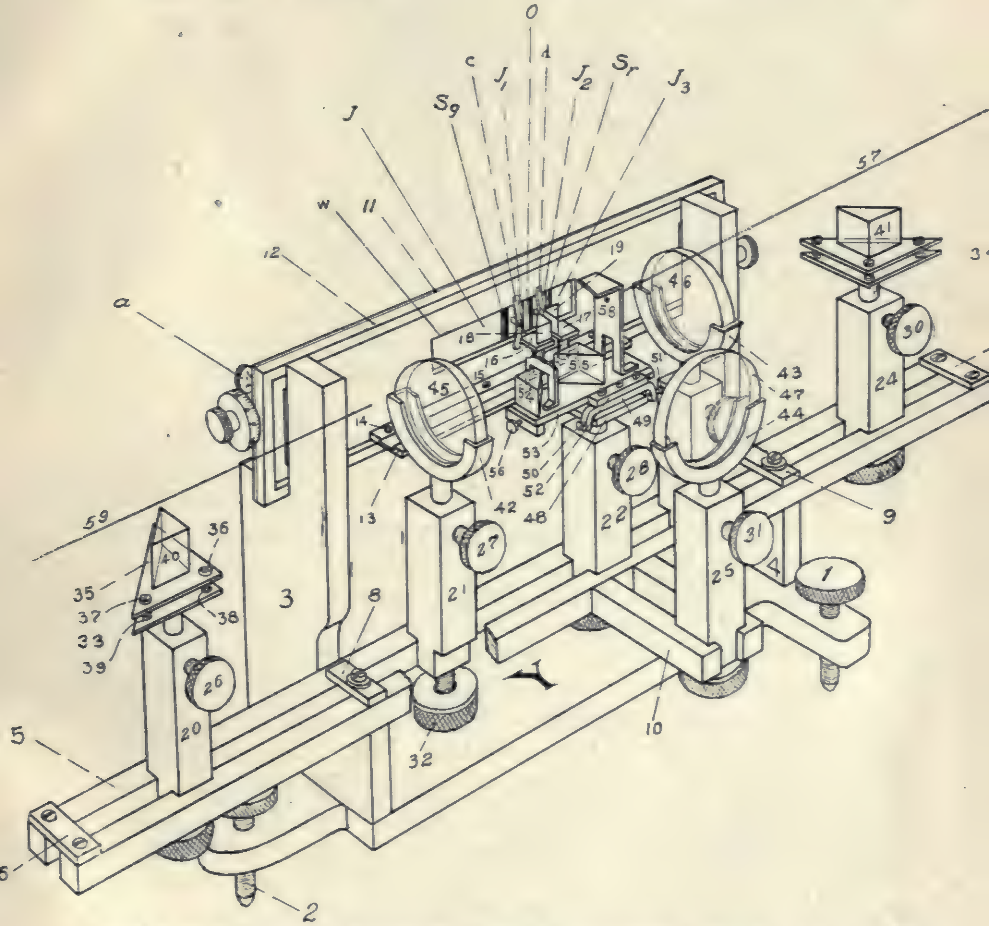


FIGURE 9—Perspective of the color selecting, spacing and reversing device. The selected bands are admitted through the slit openings, Sg and Sr. They are brought near together by total reflection prisms 18 and 19, reversed by prisms 54 and 55 and focussed by the small lenses 45, 46 and 47. The beams are spaced by the prisms 40 and 41.

23, 24, 25, which are first shouldered on their lower end so as to fit the opening between the bars forming track 5, and are then turned down to form a $\frac{3}{8}''$-12 screw. A knurled nut, 26,

serves to clamp the post to the track. A gasket is placed between nut and track.

These posts are drilled with a $\frac{3}{8}''$ hole, and are supplied with clamp-screws, 27, 28, 29, 30, 31 and 32. Posts 20 and 24 carry prism tables, fixed directly to an upright $\frac{3}{8}''$ in diameter. 35 is another plate of the same shape and size mounted above 33 by means of three screws (clearance holes are drilled for these) two of which are seen at 36 and 37. The plate 35 is constantly pressed upward against the screw-heads by means of spiral springs, two of which are seen at 38 and 39, through which the screws run to the tapped holes in 33. It will be seen that this device enables one accurately to level plate 35, upon which is fixed the total reflection prism 40. The table on the opposite side, 34, is identical in structure.

Each of the uprights, 21, 23 and 25, carries a lens holder, 42, 43 and 44 respectively. These holders carry the three small achromatic projection lenses, 45, 46 and 47.

Post 22 supports the reversing mechanism, which consists of the fixed plate 48 mounted directly upon a $\frac{3}{8}''$ rod. An upper plate, 49, is hinged to this plate by means of pivot screws 50 and 51. The upper plate has a leveling device attached, similar to that just described for plates 34 and 35. The upper plate has two beveled strips, one of which is seen at 52, screwed to it so as to form a track which receives the sliding plate 53. The two 19 mm. total reflection prisms 54 and 55 (m_5 and m_6 of fig. 6) used in reversing the beams are cemented to this sliding track. The lateral movement of the slide is limited by two stops, one of which, 56, is adjustable. The sliding plate itself, 53, is moved from side to side by means of the light gut cord, 57, attached to a metal bridge, 58. This bridge is broken away in front for the sake of clearness. The opposite attachment for cord 59 is not shown. It will be remembered that this cord system is attached to block 46 (see p. 66).

a. The double-slit mechanism and calibration.—Fig. 10 is a horizontal drawing of the double-slit structure. It is constructed as follows: A heavy brass plate, A, 3-16″ thick, $2\frac{3}{4}''$ wide and 12″ long, is attached by screws, 1–9 inclusive, to a frame made of $\frac{1}{4}''$ square brass. The plate is slotted at Sl and Sl_1, so that it may be attached vertically by thumb-screws to the planed surface of the uprights of Y (page 61). A window

wide enough to allow the whole spectrum to pass through, 1 cm. high and 8 cm. long, is cut in this wall. A part of this window is shown at O. The window is seen more clearly in the back view of the mounted slit, shown at W in fig. 9. The two thin metal strips upon which the scales Sc and Sc_1 are ruled, 6 mm. wide, 1.5 mm. thick and 20 cm. long, are beveled at an angle of 45°, and are attached above and below the window in such a way as to form a grooved track for holding the slit jaws. This track is indicated at 11 and 12. Two large slit jaws, J, and J_3 knife-edged along their vertical surfaces, slide in this track. Attached to these jaws are two shouldered nuts, K and K_1. By turning the calibrated screw-heads Cal and Cal_1, the two jaws may be made to advance or to recede. Without the addition of jaws J_1 and J_2 the apparatus may be used as a single optical slit. When these two jaws are inserted, a double slit is provided which allows two beams of monochromatic light to be admitted. These two jaws are also grooved to slide in tracks 11 and 12. They are held in their position by means of small vertical spring clips (c and d in fig. 9), fastened firmly to the center of each in such a way that the two ends of the spring work constantly against the back face of the slit-wall. The clip, combined with their snug fit in the grooved track, holds the jaws quite firmly in a vertical position. The jaws J and J_3 are provided on their front faces with indices, I and I_1, fig. 10, which give their position with respect to the scales Sc and Sc_1 respectively. These scales are needed for controlling the width of the slit and for setting the openings at desired positions.

After the apparatus has been permanently installed, it is very desirable to calibrate the double slit in terms of wave-lengths. For this purpose it is best to rule the scale Sc_1 for the whole of the length of the strip. With the slit in position one then burns in the lamp (using the *arc* between the two carbons, instead of the crater of the positive carbon as the *source*) a positive carbon soaked in a strong salt solution. A well-defined D-line appears, the position of which is noted on the scale. Several other metallic salts, e. g., lithium and barium, are then successively burned in the same way, and their lines noted on the scale. Some ten or twelve such determinations are sufficient to enable one to plot the wave-length curve of the

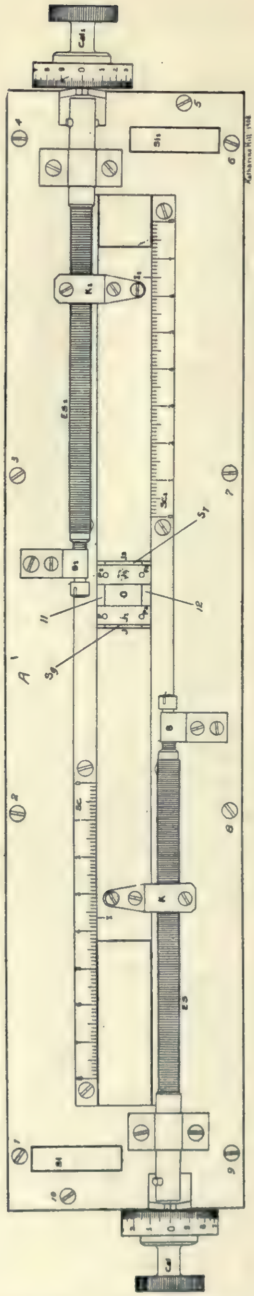


FIGURE 10—Double slit mechanism. The slit openings are shown at Sg and Sr. The width of these openings is controlled by the calibrated screw heads Cal and Cal[1]. O is an opening variable in width. Its width depends upon the spectral regions which the slit openings admit.

whole spectrum in terms of the divisions on the scale, in exactly the same way as the curve for the scale of every spectrometer is plotted. The mercury arc gives very great assistance in this calibration. The wave-lengths admitted by any given opening can be immediately read off without the use of a spectrometer. Since several of the solar lines are also clearly visible upon the polished jaws of the slit, all one needs to do when the solar spectrum is used is to make the D-line of the solar spectrum coincide with the D-line of the spectrum obtained by the use of the brine-soaked positive carbon. The distribution of wave-lengths in the solar spectrum will coincide with that of the arc. Since the D-line can be so conveniently used, the apparatus should be tested every week or so, to see that the D-line always falls upon the same scale division.

In the actual setting of the jaws so as to admit the passage of the monochromatic beams through the slit, the following method must be used: In fig. 10 the jaw J, as shown by its index, stands at 3.65. Suppose we desire to admit some other portion of the spectrum, say that which would be admitted when the scale should read 6 for J and 5.9 for J_1 respectively; we bring the jaw J back until the index reads 6 or more. The small jaw J_1 is then moved up by hand to touch J. J is then screwed forward by the micrometer screw-head Cal until the index reads 5.9. J is then brought back until its index reads 6. An optically perfect slit is thus formed, 1 mm. wide. After J_3 and J_2 have been set in a similar way, an opening, O, is left between J_1 and J_2. This opening must be closed with black oiled skin, thin soft black leather or blackened tinfoil, or by some other pliable opaque material. Four little projections are to be found upon J_1 and J_2 for this purpose. They are marked P, P_1, P_2, and P_3. This strip should be fastened on before J_1 and J_3 are withdrawn from J and J_2 respectively. In the diagram the two slit openings are shown as Sg and Sr.

The jaws J_1 and J_2 are each 5 mm. wide; consequently, with these two jaws two places in the spectrum lying closer than 1 cm. cannot be obtained. A series of single strips, knife-edged on both their vertical surfaces and beveled at their ends, should be made up. These strips should vary in width, starting at 1 mm. and increasing by .5 mm. up to 1 cm. Each should be provided with a spring clip at the back narrower than the front

width of the strip. When these are to be used, J_1 and J_2 are removed, and any desired one of these strips inserted in their places. Such a series, probably, will be adequate for the determination of the qualitative D. L. in animals.

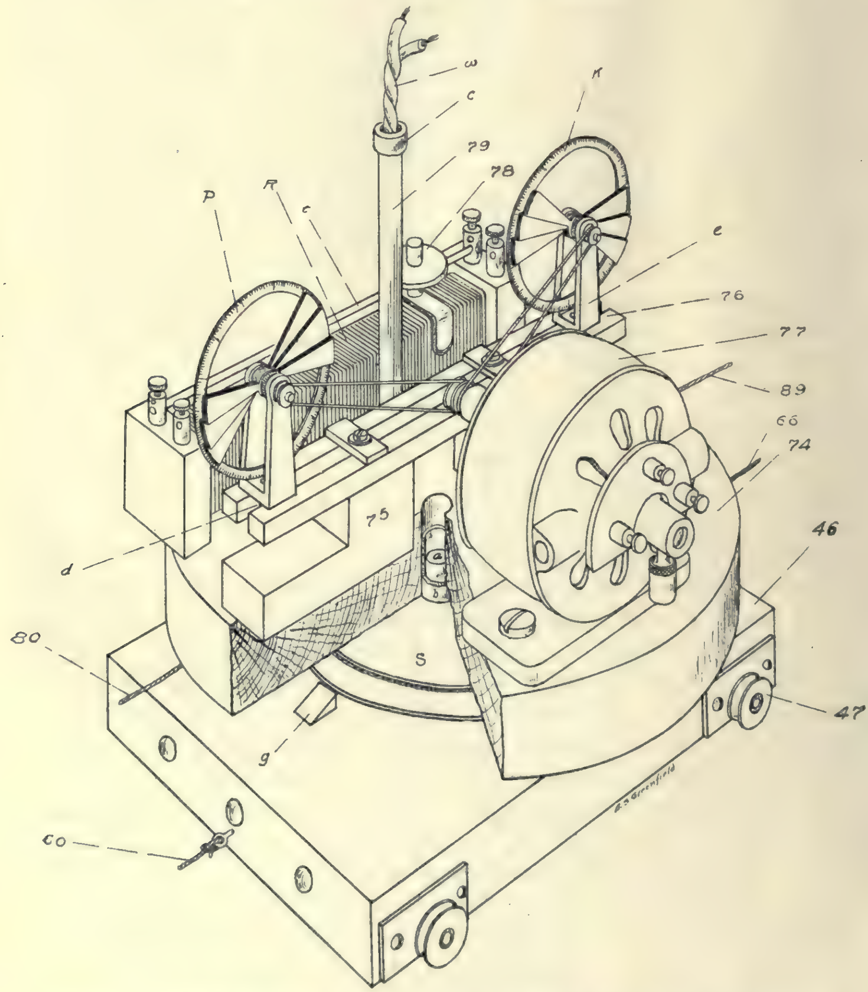


FIGURE 11—The episcotister table. The two rotating sectors are driven by the motor 77. The speed of the motor is controlled by the sliding resistance R. The whole apparatus is mounted upon a rotating table, 74, the rotation of which is controlled by a set of cords, in the hands of the experimenter, passing around the drum S. The position of the sectors with respect to the two beams is determined by the position of the carriage 46.

b. The episcotister table.—Fig. 11 is an enlarged drawing of the episcotister system shown upon the trestle work, E, of fig. 7. In the center of block 46 is fixed a vertical shaft or pivot, a, about which the circular table 74, with everything on it, is free to rotate. To the under surface of 74, and concentric with it, is fixed a wooden drum, S, 15.9 cm. in diameter. This carries a turn of gut cord, the ends of which are shown at 80 and 89. These cords are attached to H, fig. 7, and used to rotate the episcotister table. The method of winding the cord around this drum is shown separately in fig. 12.

The hole through the center of the table, 74, and the drum, S, is bushed with a brass tube, b, to prevent wear. In the illustration the bushing is partly cut away to show the pivot inside of it. Two L-shaped wooden blocks, one of which is shown at 75, support a metal track, 76, made of two ½″ brass bars, 30 cm. long, clamped to the wooden block ½″ apart. The episcotister frame **e** is locked to the stand by a clamp screw **d**. The other parts of the apparatus are as follows: K and P are two Zimmerman episcotisters 10 cm. in diameter. 77 is the motor already referred to on page 55. It is supplied with a special pulley containing two grooves ¼″ apart for receiving the belts from the two episcotisters. R is an ordinary sliding rheostat, with slides shown at 78. 79 is a ½″ brass tube, 12″ high, bushed at **c**, to receive the feed-wires, **w**. The ends of the cords attached to block 46 and to the carriage H of fig. 7 (to produce the lateral motion) are shown at 60 and at 66 on the opposite side. The working of this cord system has already been explained (p 54). At **g** is a small triangular wooden block, which prevents the cords encircling the drum S from slipping between the drum and the block 46. There are two others not shown in the figure. Finally, at 47, is shown one of the four ball-bearing pulleys, upon which block 46 travels.

In fig. 12 is shown the winding of the cord system and the mechanism for rotating the episcotister table. The disc shaded with the diagonal lines is the drum S spoken of in connection with fig. 11. The positions of the motor, M, and of the episcotisters, P. and K, are indicated. A, Fig. 12 shows how the cords (a single continuous cord is really used) are fastened to one point of the drum circumference before the free end of 89 is wrapped around it. B, fig. 12, shows that cord 89 is given one turn

around the drum. The two ends of the cord are then forced to run over fixed pulleys and are joined as described on page 56 and made to run freely through a hole in the metal bar supplied with a clamp-screw, 87 in fig. 7. It must be remembered that block 46 is free to slide on its track from right to left. Suppose that block 46 is pushed to the left, and that the cord around the drum is locked at 87; fig. 12, C shows what will happen: first, note the position of the episcotisters in B, fig. 12, and compare this with C, fig. 12, where block 46 has been made to travel one-half its distance from right to left. It will be seen that the drum 74 has been forced into rotation by the unwinding of 89. At the same time, cord 80 has been made to wind up. Note that in C, fig. 12, motor M and the two episcotisters have moved one-fourth of a turn clockwise (in which position the episcotisters no longer interrupt the beams). In fig. 12, D, block 46 has reached its extreme left position. Each cord is now half wound up. The episcotisters are now reversed (as are the right-left relation of the two colors). In fig. 12, B, (corresponding to position shown in fig. 7) the green is on the right and is interrupted by episcotister K, whereas the red beam is interrupted by P. In fig. 12, D, the green beam is on the left and is still interrupted by K; whereas the red is on the right and is still interrupted by P. Referring again to fig. 12, B, suppose that the cords 80 and 89 are not fixed and are allowed to move freely through the clamp 87, fig. 7; as block 46 is made to travel as before to its extreme left position at fig. 12, D, the cords 80 and 89 will not wind and unwind as they did in the other case, but will slip easily over their pulleys. The table 74 will not rotate, and consequently the episcotisters will interchange with respect to the two beams. In this case at fig. 12, B, P will engage the green and K, the red.

As might be inferred from the above description and from that on p. 56, three possible ways of using the mechanism are open:

(a) The sectors may be turned until they take the position fig. 12, C, the cord being allowed to run freely through the clamp 87, fig. 7. The full intensity of the beams is available.

(b) The same sector can be made to interrupt a given beam regardless of the latter's left-right position (in order to effect this turn the sectors to the position B, fig. 12, and lock the cord at 87, fig. 7). This adjustment must be open to us when we are

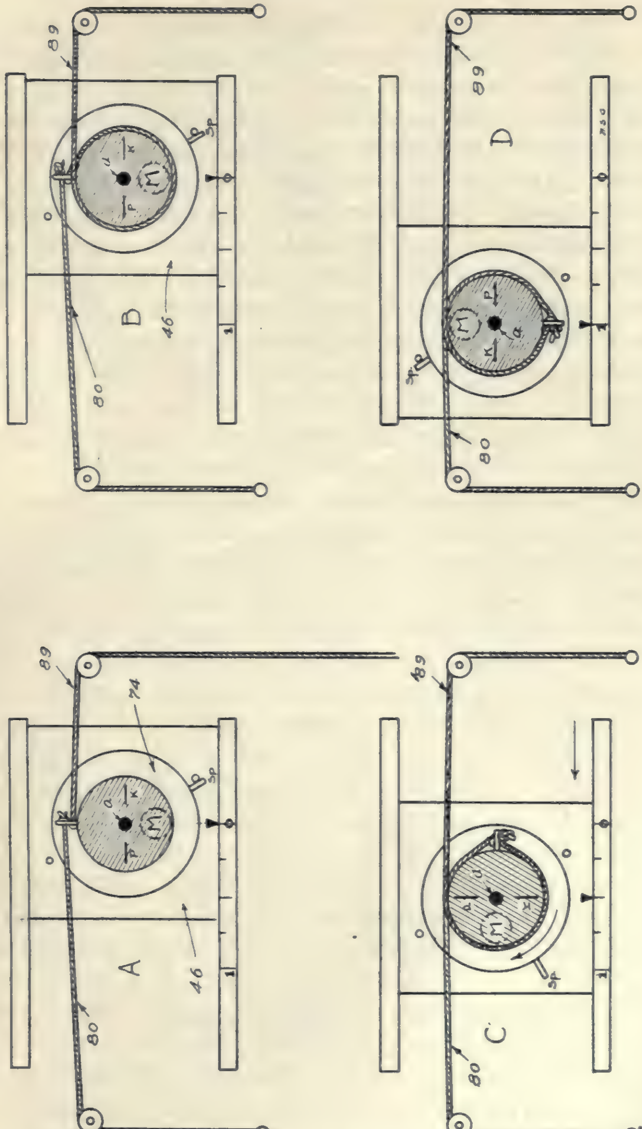


FIGURE 12—A schematic drawing of the mechanism for rotating the episcotister table. The rotating table of fig. 11 is marked here by the number 74. The shaded circular area represents the drum S of the previous figure. The motor is indicated by M, the sectors by P and K, the cord system by 80 and 89. A shows the method of attaching the cords; B, C and D show the different positions of the sectors.

working with a fixed and known difference in energy between the two beams. Where quantitative relations are to be thus maintained, special sectors, described on p. 76 must be used.

(c) The sectors can be made to interchange with respect to the two beams (turn sectors as before to position B, fig. 12, but do not lock cord at 87, fig. 7). This is desirable where abrupt changes in brightness are to be introduced. For this work the Zimmerman sectors are best. One of the sectors can be set with a wide angular opening, the other with a small opening. The intensity of the two beams is thus automatically changed with every change in the left-right position of the beams. Care must be taken in making this setting that both of the colors remain clearly supra-liminal for the animal when the sector with small angular opening respectively engages them.

4. Description and cost of parts of apparatus

a. Sources and their control.—Sunlight and heliostats. A good heliostat greatly increases the ease with which sunlight may be used. There is no cheap and satisfactory heliostat on the market. Nearly all of them are supplied with clockwork which is too weak to drive them steadily. They are usually made too light to withstand gusts of wind. Furthermore the mirrors attached to them are usually silvered on the back. This enormously decreases the intensity of the reflected beam and gives rise to a series of disturbing secondary images. The mirrors may be silvered on the anterior surfaces, but the silver film deteriorates rapidly.

The Fuess heliostat is a convenient one. It permits of use in any latitude and will reflect the beam in any direction. It is listed by Max Kohl (supplied through the Arthur H. Thomas Company) at $125. At the Johns Hopkins University Professor Anderson has modified the original castings of Professor Rowland, and has devised a massive heliostat supplied with two heavy speculum mirrors. These mirrors are ground and polished by Brashear to a surface equal to that of good plate glass, which is accurate enough for the requirements of most work on vision done in psychological laboratories. This heliostat can be supplied for about $150.

Arc lamp.—The automatic arc of Bausch and Lomb with adjustable rheostat of 25 amperes capacity is especially recom-

mended as the standard source in all general high intensity work. This lamp, supplied by the Arthur H. Thomas Company, Philadelphia, complete for $102, is simple in its mechanism. The feed is purely magnetic. As the two carbons burn away they offer greater and greater resistance to the flow of current; finally a portion of the current is shunted through the feeding mechanism. The carbons are then fed toward each other until the resistance becomes so small that no further current passes through the feeding mechanism. The forward motion of the carbons, therefore, ceases before they are brought into actual contact. The lamp is always practically noiseless, with the exception of the "cluck" or series of "clucks" made by the feeding mechanism. A drop of from 1 to 3 amperes is sufficient to start the feednig device.

It was long ago shown by Abney that even a large decrease in amperage does not alter the intensity of the crater of the positive carbon. *It merely decreases the size of the crater.* Since the height of the slit used is less than the diameter of the image, it follows that the variation in the energy of the monochromatic bands is hardly detectable. Of course there are slight imperfections in the carbon, which occasionally produce a sudden flicker and slight shiftings of the positions of the carbons, but such disturbances are *momentary* and *negligible.* It is essential to have a *large crater.* This can be obtained only by the use of a high amperage. Twenty-five amperes is a necessity. A positive cored carbon (Elektra) 9-16″ in diameter and a cored negative carbon 7-16″ in diameter are very satisfactory. If other sizes of carbons are used, or if the amperage is low, there will be a shifting of the crater and a tendency for the two carbons to burn unevenly. Suitable carbons may be purchased with the lamp.

These lamps are very durable. One of them at the Johns Hopkins University has now been in operation two years and gives entire satisfaction. It burns for 1½ hours without change of carbons. It has burned daily for months without attention and during that time has never jammed the carbons nor in any other way interrupted an experiment.

If energy measurements are to be made, it is essential to have both an ammeter and a voltmeter in the circuit with the arc. The voltmeter should read 0–130 by 1-volt divisions.

(It is assumed that the current used will be D. C, ranging between 108 and 120 volts.) This voltmeter will show the general steadiness of the power-house or city circuit. The city current should be used at the time of day when it is most steady. The ammeter should read 0–25 amperes. These instruments need not be of expensive type. Jas. G. Biddle, Philadelphia, has satisfactory instruments, listed as the Keystone type C. voltmeters and ammeters. The cost is about $15 each for voltmeter and ammeter.

In using the arc with the Bausch and Lomb adjustable rheostat admitting the maximum of current it will be found that the amperage is at first rather high, but that as the lamp and resistance box gradually heat up the amperage drops to 23 or less. This increased resistance soon reaches a maximum, however, if the room is well ventilated. The arc will thereafter burn steadily within fixed limits of amperage.

It is advisable to take off the casing of the Bausch and Lomb lamp and to study the mechanism in action. It comes already adjusted but there is a small "vibrator" or interrupter at the bottom of the lamp which may need adjusting. This is the chief source of trouble in the lamp, but it is not serious. The small feed wires connected with the negative and positive carbon receptacles are too small as they come with the lamp, and are likely to burn out, especially if the carbons are not screwed in tightly. It is desirable to replace these with slightly larger wires if this mishap occurs. The lamp is supplied with both horizontal and vertical adjustments. The positive carbon is put in line with the optical axis of the system, and leveled with an ordinary spirit level.

The Nernst filament. The Nernst glower is the most satisfactory form of source where a spectrum of medium to low intensity can be used. It is the only source suitable for stimulus limen (R. L.) and stimulus difference (D. L.) work. A card containing 12 filaments, .8 amperes, 110 D. C., may be purchased from the Nernst Lamp Company, Pittsburg, Pennsylvania, for $3. In addition to the filaments a small "ballast" (resistance) must be burned in series with the filament. This is supplied for 50 cents. In doing quantitative work this filament should be burned in the circuit with a volt- and ammeter. The voltmeter just described is adequate but since the amperage is small, an ammeter reading 0–1 (or 0–5) should be used; this

likewise can be purchased from Jas. G. Biddle, Philadelphia, for about $15. The Nernst filament is used by some experimenters simultaneously as source and slit. Others mount it immediately behind the slit (e.g., S, fig. 6). These two methods of using the Nernst are unsatisfactory for continued use, for several reasons which need not be entered upon here. The best method of mounting is that suggested on p. 47.

Slits.—The first slit in the optical system, S, fig. 6, is an ordinary cheap form: (No. 1251 in Max Kohl's catalog.) It is supplied through the Arthur H. Thomas Company, Philadelphia, for about $8. The slit as it comes from the factory is greater in height than is necessary for the present work. It is best to limit the height to about 2 mm. by the use of auxiliary metal strips at the top and bottom. These should be knife-edged in the usual way. The width of the slit is controlled by a micrometer screw.

The double slit-wall, fig. 10, is made from our own design by Wm. Gaertner, of Chicago, for about $45. If it is desired the parts supported by casting Y, fig. 9, can be furnished complete by the Johns Hopkins University. This includes the casting itself, the whole of the double slit mechanism, prisms, prism stands, lenses, lens holders, etc. The cost will be about $100.

b. *Lenses.*—Suitable lenses of large aperture and short focal length are very hard to obtain for the apparatus at a reasonable price. Many of the large photographic houses in Boston and New York deal in second-hand portrait lenses. In equipping the psychological laboratories of Harvard, Chicago, Illinois, and Johns Hopkins, very satisfactory lenses of this type have been purchased, at prices ranging anywhere between $5 and $35. It is not difficult to get portrait lenses of 3″–4″ aperture with long focal length, but these must be avoided if an intense spectrum is to be obtained.

To obtain satisfactory *condensers* is especially difficult. Probably the best and easiest solution of the problem is to get Bausch and Lomb to grind these lenses as single achromats. A single achromat of 3½″ aperture and 6″ focal length is recommended. The cost of this lens unmounted is $30. The two elements of the condenser should not be cemented, but mounted with an

air-space between. Four such lenses have already been ground to our order. They are very satisfactory.

After this lens has been installed it should be carefully protected from a possible discharge of particles from the arc by a metal cap. The danger from the arc comes when it is first started and when the carbons have "frozen" and suddenly burn apart. After the arc has begun to burn steadily the cover can be removed. There is little or no danger of breakage from heat.

A compound portrait lens is best for the *collimator.* The Voitländer, if it can be obtained with short focus, is very satisfactory. There are several of these on the "second-hand" market. They should not be over 12″ in focus nor under 3″ clear aperture.

The *objective* should be of the compound type if possible. A large compound lens from a camera or stereopticon can be used. The clear aperture should be at least 3½″ in diameter and the focal distance not over 50 cm. Several Steinheils may be obtained which are accurate enough for all the work recommended in this report. They may be had of Robey-French and Company, Boston, or from Chas. G. Willoughby, 814 Broadway, New York.

It is well to test the lens before purchasing, by mounting it as a camera and allowing the image of a series of long, accurately ruled cross lines to fall on a ground glass plate. These lines should all appear in the image equally intense and undistorted. Many of the compound portrait lenses on the market give images which are not sharply defined. This "softness" in definition is an advantage in photography but it is a serious defect in an objective for optical work.

Several small achromatic projection lenses are needed—three behind the double slit, one for the box containing the Nernst filament (see p. 50) and one as a condenser when the Nernst is used in place of the arc. For these purposes we have used small field or opera glasses. Opera glasses costing $3 or $4 a pair will afford suitable lenses. They should be about 3¼″ to 4″ in focal length and should have an aperture of about 1″. They are to be entirely removed from their mountings and remounted as shown in fig. 9. The Wollensak Optical Company, Rochester, N. Y., will grind small single achromats to

order at a very reasonable price. The small lens used as a condenser with the Nernst filament (p. 47) and the small projection lens referred to on p. 51 were furnished by them for $1 each. The lenses of the system need not cost over $100.

c. Prisms.—The *spectrometer*, or refracting, *prism* recommended may be had of O. L. Petitdidier, Lake Ave. and 53d St., Chicago, or of Hans Heele (Arthur H. Thomas Company, Philadelphia). Heele furnishes a dense flint 60° prism 80 mm. aperture (ref. ind. for D=1.70) for $60. (Extra dense flint, ref. ind. for D=1.78 for $68.) Petitdidier furnishes a very satisfactory prism 4″ x 4″ face for $50, another 3½″ x 3½″ for $30. He rarely carries these sizes in stock, but will make them to order.[1]

This prism should be mounted on a metal table 5″ in diameter (see p. 7 fig 7). It should be set in minimum deviation for some clearly marked line easy to obtain in sunlight or arc, e.g., the D line, and never thereafter moved. Care must be taken to level the prism very accurately after it has been installed. For this reason it is best to have the top of the prism table made in two parts after the manner described on p. 62.

Small *total reflection prisms* of flint or crown glass may be purchased of Queen and Company, Philadelphia; O. L. Petitdidier, Lake Avenue and 53d St., Chicago, or Bausch and Lomb, Rochester, N. Y. Queen and Company furnish them most cheaply and with all the accuracy required for the present work.

Two 9 mm. prisms are needed immediately behind the double slit. These may be had for $1.75 each. Two 19 mm. reversing prisms are required, $2.50 each; and two 25 mm. spacing prisms, $3 each.

A small compound double image prism, a sketch of which is shown on p. 49 is necessary for use in experiments on the liminal difference for intensity (D. L.). This may be had of Steeg und Reuter through the Arthur H. Thomas Company, Philadelphia. The cost of this prism is about $30.

All prisms should be cemented permanently to their respective tables. Several cements are on the market, but a most satisfactory cement can be made by taking dry shellac and pouring a small amount of 95% alcohol in with it, warming the mixture over the flame until a homogeneous, fairly thick paste is formed.

---

[1] A prism with high refractive index is very likely to tarnish. Mr. Petitdidier recommends a prism with refractive index not greater than 1.66.

The metal surface should be warmed quite thoroughly and then spread quickly and evenly with the shellac. The prism should be warmed also and then firmly pressed down upon the metal surface. After drying (for about 2 hours) a satisfactory union is obtained.

d. Episcotisters; and motor iris diaphragm.—An episcotister supplied with movable sectors has its advantages and its disadvantages. Its chief advantage consists in the fact that it may be set so as to cause only a small loss of energy in the beam. It is made with a rim of metal graduated in degrees, which is attached to the rotating shaft by two fixed sectors of 21° each. The rest of the sectors are free to move concentrically on the shaft, where they can be clamped by a lock nut in any desired position. They can thus be made to close the whole 360° opening, or folded in behind the two fixed sectors in such a way that the angular opening reaches its maximum of 318°. They can be arranged thus to give about 88 per cent. of the full intensity of the beam.

Its chief disadvantage consists in the fact that there are so many sectors that an accurate adjustment of the angular opening cannot be made. In making threshold tests or tests of the DL for intensity and in accurately adjusting energy relations of any kind by the use of the selenium cell, the episcotister supplied with movable sectors is out of the question. It is, however, extremely valuable for qualitative work, where one desires quickly to present a color stimulus first with high, then with intermediate and finally with low intensity. Zimmerman, through the Arthur H. Thomas Company, Philadelphia, furnishes three sizes of adjustable episcotisters—10 cm. opening, 38 marks; 15 cm. opening, 50 marks; 20 cm. opening, 65 marks. The first two sizes are especially adapted to our work.

In order to make a satisfactory sector for use in all quantitative experiments one uses the frame work, shaft, pulleys, etc., of the one just described, but unscrews the rim with its two fixed sectors and takes off the free sectors. Two sectors are then prepared in the following way: Two discs of brass 10 cm. in diameter—or 15 cm., depending on the size of the Zimmerman episcotister—are drilled at the center to fit the shaft of the episcotister. The one of these discs taking the place of the rim and the two fixed sectors of the above, should

be 1-16″ in thickness and the other should be 1-32″. Each of these discs should be laid off and cut out as follows: Two diameters are marked out at right angles to each other. Opposite quadrants of the four thus formed should be sawed out, leaving an outside rim of 5 mm. and an inside rim of 10 mm. at the axle. One of the quadrants should be graduated in degrees. This work should be done with great accuracy. The heavier of the two discs is then screwed into the bushing on the axle which formerly held the fixed disc of the episcotister. The thin disc is slipped on in place of all the movable sectors. It is clear that the two opposite open quadrants of the movable sector can be made to coincide either with the open or closed sectors of the fixed disc. In the one case is given the maximum opening of 180°, and in the other the minimum, 0°. The graduation on one of the quadrants shows one-half the total opening always.

When it is remembered that the energy of an interrupted beam of light is directly proportional to the angular opening of the rotating sectors, it is seen that when once the full energy of the beam has been determined by means of the selenium cell, the energy of the interrupted beam becomes known from the angular setting of the episcotister. It is possible thus to make liminal and differential threshold-tests with accuracy.

In making settings of the episcotister of less than 2° (i. e., of less than a reading of 1° on the scale), a graduated wedge should be used, permitting an opening as small as 1-100° to be made with accuracy. In making threshold tests this opening will have to be obtained by the use of an angular slit mechanism controlled by a micrometer screw. The opening can be set with an accuracy of 1-1000 mm.

Any small high speed motor may be used to drive the episcotisters. Motor No. 5700, 110 D. C., 1-20 H. P. of the C. H. Stoelting Company, Chicago, costing $15, is very satisfactory. The circular starting box and resistance usually supplied with it should not be used, but in its place should be substituted a sliding rheostat of suitable winding (75 ohms). James G. Biddle, Philadelphia, supplies a duty free "G. R." rheostat, type E. H., 75 ohms resistance, of excellent construction, for $5.36.

Slender circular leather belts, glued together and sandpapered until they are perfectly smooth running, should be used for driving the sectors. Lacking these, rubber bands may be used. The latter work well for a time, but soon deteriorate. It is well to keep a supply of them close at hand for emergency use.

Iris diaphragms [1] with maximal diameter of 2.5 cm., suitable for mounting upon the small projection lenses shown in fig. 9, may be obtained of Spindler and Hoyer through Arthur H. Thomas Company, Philadelphia, for 5 marks. They are not very accurate at best, and their use is recommended only as an additional means of control of intensity. Had we proof that the interruption of a beam of light by the rotating sector alters the intensity of light in the same ratio for the animals as it does for the human being—i. e., according to the law of mean squares—there would be no necessity for any other form of control of intensity. Lacking the actual proof, however, it seems best to have occasional recourse both to the iris diaphragm and to the smoked wedge.

e. Mirrors.—Speculum mirrors are far more satisfactory in this work than any other form. They are expensive, however, and difficult to obtain. The expense is due largely to the difficulty in manufacture. A model for the mold must first be made in wood, from which a metal casting is made. The casting is then accurately planed. The speculum metal is melted and poured into the mold. A constant stream of illuminating gas is made to enter the mold while the molten mass is being poured in. The gas ignites and consumes all the air present in the mold. The plates are removed from the mold and placed in a bed of burning coke for several hours, in order to anneal them properly. After cooling they are ready for grinding and polishing. In the work we are attempting to do there is no necessity for obtaining a planeity of surface greater than that offered by the best plate glass.

The three large mirrors 3″ x 5″ called for in this apparatus were cast by Mr. Childs, and ground by John A. Brashear of Pittsburg. The total cost of each mirror is about $15. Max

[1] Since the openings in the double slit wall are in the foci of these lenses it follows that the size of the images falling upon the plaster of paris surfaces is not altered by the use of the diaphragm.

Kohl charges a much higher price—400 marks for the 4″ x 5″ size; 300 marks for the 3″ x 3″ size.

These mirrors with care are almost absolutely permanent; several speculum metal gratings made by Professor Rowland in 1890–95 show no signs of deteriorating. If through neglect the mirrors do become somewhat dull, they can be repolished by using a fine grade of prepared chalk, moistened in distilled water or alcohol. One applies the chalk and polishes with absorbent cotton kept wholly free from dust. If after long disuse the surface of the mirror appears stained, it can still oftentimes be brought into first class condition by the application of dilute C. P. ammonia. This also is applied with absorbent cotton. After using the ammonia the mirrors are cleansed thoroughly with distilled water, and then polished with prepared chalk as before. Finally if through neglect the surface has deteriorated beyond the point where the above simple remedies will restore them, they can always be repolished by Brashear at a fractiou of their original cost.

Professor Anderson of Johns Hopkins has devised a method whereby mirrors of a permanent kind can be made by depositing various metals possessing high indices of reflection, directly upon plate-glass by an electrolytic process. These mirrors may now be obtained in gold, silver, nickel and platinum. They are far less expensive than speculum mirrors, and make a satisfactory substitute for them when expense has to be considered.

f. Instruments for measuring wave-length and intensity.—Selenium cell and auxiliary apparatus.—Professor A. H. Pfund has devised for us an exceedingly delicate method of measuring the energy of the various monochromatic bands *after being diffused by a ground glass or plaster of paris surface.* He gives the following brief description of the method: "In order to measure the energy carried by a beam of monochromatic rays, some bolometric device is usually chosen. When the amount of energy to be measured is so very minute, the sensitiveness of the best bolometer is found insufficient, and recourse must be taken to some other form of energy-measuring device. The wave-length interval over which the present work is to extend lies within the visible spectrum, and since it is precisely in this region that the selenium cell is so very sensitive, it was chosen above others to serve as a radiometric device.

" To determine whether or not an animal has color vision, it is essential, in testing the response of the animal to two differently colored beams, that the energy carried by these two beams be the same—i.e., that they differ in wave-length only.[1] This control is obtained by the use of a selenium cell whose color sensibility curve is known. For the details of the method used in determining such sensibility curves the reader is referred to the Physical Review, vol. 28, p. 324. Suffice it to say here that the sensibility curve is plotted between galvanometric deflections caused by changes in resistance of the selenium cell, and the wave-length of incident beam, the energy carried by each bundle of monochromatic rays being constant. Given the sensibility curve of a cell it is evident that we have the means at hand of determining the condition under which two bundles of monochromatic light carry the same energy.

" Selenium cells obtained from different makers do not necessarily have the same sensibility; hence it is necessary that each cell be calibrated as above indicated. Furthermore it is required to know in terms of C.G.S. units the energy carried by the light used in the excitation of the cell. This is necessary in order that the result obtained by any one investigator may be strictly comparable with those of any other investigator. Although the selenium cell connected in series with two or three dry batteries and the galvanometer may be used when the amount of energy to be measured is large, it is best to use the cell as a part of the simple potentiometer, thus placing all possible degrees of sensitiveness at the immediate disposal of the investigator. The following plan is suggested for the standardization of this work:

"A central bureau is to be decided upon, where the calibration of all instruments is to be carried out. The color sensibility curve for all cells is to be carried out in the wave-length interval 450–800, and for the same absolute intensity of light. Lower intensities than that at which the cell is calibrated are obtainable through the use of a rotating sector. Due to the fact that the selenium cells, galvanometer, batteries and resistances will not

[1] This is only one of the reasons for an energy control. We desire oftentimes to present the two beams when they differ very markedly in energy, and for the sake of reproducibility of conditions it is essential to know the energy of the beams at all times.

be identical in all laboratories, and further to the fact that all the above instruments are affected in their behavior by temperature changes, it is necessary to provide for some method of standardization which will make it possible to employ the cell always under the conditions which obtain while the cell is being calibrated. This method is briefly as follows: While the sensibility of a given cell is being determined, it is noted that under the influence of green light ($\lambda = 500\,\mu\mu$) a deflection of 60 mm. is obtained. The cell is then exposed to the total radiation of the standard 8 c.p. incandescent lamp, which is removed to such a distance that the deflection produced is also 60 mm. Next, the cell is exposed to another similar lamp, burning as before at an accurately measured voltage, and this lamp is also removed to such a distance as that at which a deflection of 60 mm. is obtained. This second lamp accompanies the selenium cell. When it becomes desirable to use the cell again it is connected with the galvanometer and potentiometer and is exposed to the incandescent lamp which is burning at the same voltage and at the same distance from the cell as at the time of standardization. If then the galvanometer does not record a deflection of 60 mm. its sensibility is varied until such a deflection is obtained. However true it may be that the manipulation of the selenium cell is a trifle complicated, it is to be noted that this objection is more than off-set by the tremendous sensibility placed at our disposal."

The apparatus for measuring energy consists of the following parts: (1) A selenium cell of 2,000,000 ohms resistance, supplied by J. W. Giltay, Delft, Holland, through Arthur H. Thomas Company, Philadelphia, for $20. (2) A Leeds and Northrup type H wall galvanometer with a low resistance coil (31 ohms), supplied with telescope and scale, costing about $45. (3) Three Leeds and Northrup resistance boxes (1–10,000 ohms) for altering the sensibility of the galvanometer and for use with the selenium cell, costing about $24 each; these, with the galvanometer, can be had of the Leeds and Northrup Company, Philadelphia. (4) Four storage batteries, open glass cell chloride accumulator, with CT type of element, 4″ x 5″, supplied by Jas. G. Biddle, Philadelphia, at about $2.50 each.

Professor Pfund has consented to standardize a few of the selenium cells. If it is desired, the whole apparatus can be

purchased through the Johns Hopkins University. It will be sent out after being standardized, with a chart and full directions for installation.

Spectrophotometers.—While the measure of energy in the bands of monochromatic light seems to be the most desirable form of control, since it is absolutely independent of the sensitivity of the human eye, the apparatus for obtaining it, as we have seen above, is expensive, and the technique is somewhat difficult. It seems worth while, accordingly, to mention at least another standard method of control—that of spectrophotometry. This method, as is well known, measures the intensity of a variable monochromatic beam in terms of the intensity of a monochromatic beam of the same wave-length coming from a standard source. There are many complex forms of spectrophotometers on the market, such as the Lummer-Brodhun, costing about $500, the Brace, costing $375, Glan's, costing $125, but all these are more complicated than is necessary for our work.

After some experimentation we suggest the use of Nutting's pocket spectrophotometer. This is a compact little instrument, which gives an accuracy within 2-3 per cent. of the larger instruments. It is inexpensive ($31) and easy to use. It is made by R. Fuess, and may be purchased through the Arthur H. Thomas Company, Philadelphia.

The instrument needs a small tungsten lamp of known candle power attached to it at a fixed distance from the slit, e. g., 10 cm., for the purpose of giving a reference or standard spectrum. The instrument should be mounted in a metal arm running at right angles to it for convenience in clamping it to an upright. This arm is graduated in mm. and carries the standard lamp. The white light from this lamp enters the instrument through a window in the side, meets there a small total reflection prism, and is reflected through the slit parallel to the axis of the instrument; passing through the usual lenses, direct vision prism and Nichol prism (the polarizer), it is refracted, polarized and finally focussed on the vertical slit in front of the eyepiece. The width of this slit is adjustable. The variable monochromatic beam enters at the end of the instrument, passes through the first Nichol prism (the "analyzer"), thence through the lenses, the second Nichol, etc., as above. In the eyepiece one spec-

trum appears vertically above the other. The slit in front of the eyepiece is then adjusted so as to admit the whole of the variable beam. Immediately below it with no line of demarcation one finds a band of monochromatic light of exactly the same wave-length, coming from the *standard* source. The latter in this work is adjusted in intensity so that the standard beam is always fainter than the comparison. The comparison beam is then decreased in intensity by the use of the analyzing Nichol until the two are of the same brightness. The angle of the analyzer is then read, which gives after proper translation the intensity of the variable beam in terms of the standard.[1]

This method, while in general use, is subject to all the variations present in the photometry of white light. A standardized procedure, accordingly, should be adopted. Numerous determinations should be made and with a dark-adapted eye. Since they do not have to be made frequently, it seems desirable to allow at least 15-20 minutes for adaptation to darkness before taking readings.

The light chosen as a standard should be a well seasoned miniature tungsten with approximately straight filament (the bulb should be marked with fiducial lines and the lamp should be burned in a position constant with relation to the instrument) of about 1½ c. p. at 2 volts pressure. It should be burned in series with a rheostat and one two-volt storage battery. A volt meter reading 0–6 in 1-25 volt divisions should be placed in the circuit. Switchboard type C voltmeter, supplied by Jas. G. Biddle, Philadelphia, for $15, will suffice, but a precision voltmeter of the well known Westinghouse type is better.

Flicker photometer.—In making color tests upon animals, the student of behavior is not especially interested in obtaining color stimuli which offer "equality of brightness" to the human eye. This relation may or may not have bearing upon the animal work. It is always interesting, however, to test the animal under such conditions. The flicker photometer presents probably the most uniform way of obtaining between disparate colors a relation which is considered by some to be that of equality. For a thorough experimental treatment of this

[1] For further description of the instrument, see P. G. Nutting, Bull. Bureau of Standards, 1906, vol. 2, p. 317.

subject, the reader may be referred to the work of Ives, which is to appear shortly in the Physical Review.

Spectrometers.—There are many simple instruments upon the market which will give the wave-length of the bands used with sufficient accuracy for the present work. A pocket form of instrument supplied with a scale is the most convenient. The relation of the scale divisions to the distinct lines in the solar spectrum is then determined, and the "curve" of the instrument drawn on millimeter cross section paper.

The Browning pocket spectroscope is supplied by Max Kohl for 82 marks. Hans Heele also furnishes a pocket spectroscope with scale for the same price (purchase with Amici prism of high dispersion). Either of these instruments may be imported through the Arthur H. Thomas Company, Philadelphia.

We suggested on p. 63 that the scale of the double slit be calibrated directly in terms of wave-lengths. Once this has been accurately done there is no further need of a spectrometer. It is well, however, to have one at hand for use in making occasional tests.

g. Experiment box.—On account of the great differences in the structure, form, and instinctive capacity of animals which are likely to be used as subjects in these tests, no fixed type of experiment box can be recommended for use with the color apparatus. That shown in fig. 4 suggests a simple type of construction.

One of the chief difficulties in the way of using spectral light in dark-room work is the peculiarity which the shorter wave-lengths exhibit in illuminating the interior of the black experiment box. Even the darkest of our black paints reflects the green rays sufficiently to show the outlines of the floor, the partition, and the walls of the experiment box. The red rays, on the other hand, are completely absorbed by some black paints. This means that under the ordinary conditions of our tests a secondary criterion is afforded the animal. On the basis of the criterion, it soon learns to *avoid* the more highly illuminated compartment, if its food is given with the color of longer wave-length, or to *seek* it if its food is given with the color of the shorter wave-length. Two animals, a white rat and a rabbit, under experimental observation soon learned to

make 100 per cent. of correct choices, on the basis of the difference in illumination of the two compartments.

This defect in the method of presenting chromatic stimuli is not peculiar to spectral light. Any transmission method offers the same difficulty. So far as we know this defect has not hitherto been called to the attention of experimenters. The difference in illumination after 10–15 minutes of darkness adaptation is so marked that one must notice it. The phenomenon does not depend upon the difference in energy (or " brightness ") of the two colors. It is apparent even when the green is but one-twentieth the energy of the red. It is still more apparent when the two colors have been judged " equal in brightness " by direct comparison. It is due chiefly to the fact that black paints offer a surface of reflection for rays of the green-blue-violet regions of the spectrum.

After long experimentation we have found two ways of wholly eliminating it. The first method calls for the abandonment of tests with complete darkness adaptation. One mounts a 40 watt tungsten lamp in a light-tight metal cylinder, in the front end of which is fitted a graduated iris diaphragm with a disc of ground glass behind it. The diaphragm can thus be opened and closed to regulate the intensity of the light. Since the light is perfectly diffused by the ground glass, the size of the illuminated field does not appreciably change when the diaphragm is opened or closed. This light is mounted centrally over the experiment box at a height of 1.5 meters. Care must be taken that the white light offered by this source does not enter the cabinet (p. 57) and fall on the plaster of paris surfaces. With this illuminating device the two food compartments may be so highly illuminated that the relatively slight difference in illumination, arising from the source discussed above, is completely eliminated. Entirely apart from its uses in this connection, the device gives us the means of studying the formation of associations under standard conditions of light adaptation.

The second method consists (a) in mounting a series of diaphragms in front of the windows in the experiment box which admit the colors; (b) in making the floor of the experiment box non-reflecting, by means of a metal grill; (c) in fastening dead-black velvet to the sides of the box and to both sides of

the partition. These three precautions totally eliminate all difference in the behavior of the long and short wave-lengths, for the very simple reason that all the reflecting surfaces where light immediately falls have been removed.

(a). The diaphragms are made from 1-32″ sheet brass. Eight pieces 4″ x 7″ are cut out, and in the center of each a rectangular opening 2″ x 4″ is milled and knife-edged. Four such diaphragms should be mounted together and placed in front of each of the two windows in the experiment box. They should be mounted as follows: The four 4″ x 7″ sheets are clamped together and drilled at the four corners with a ¼″ hole; four ¼″ brass rods are cut, 6⅝″ long and are tapped and supplied with hexagonal nuts ¼″ thick. The four diaphragms are slipped on these rods, and are separated from one another by means of collars 2″ long, made from brass tubing, the inside diameter of which is ¼″. The set of diaphragms is acid-blackened. This method of mounting the diaphragms enables one to dispense with a supporting floor, which would offer a reflecting surface. The diaphragms when assembled should be mounted in front of the two windows in a bearing which offers universal adjustment, for ease in centering with respect to light. (As a precautionary measure it is well to cover each of the diaphragms with dead black velvet. The velvet should not be placed nearer than ¼″ to the edges of the knife edged rectangular opening.)

(b). The non-reflecting floor grill is made to serve both as a punishment device (if desired) and as a means of absorbing light. It is made as follows: (The size given is one fitting the floor of an experiment box made for small mammals. This box is similar to the one shown in fig. 4. The grill fits snugly the whole of compartment B.) Three ¼″ brass rods 20½″ long are tapped at their ends and supplied with nuts ¼″ thick. The whole rod is bushed with wood-fiber tubing, the outside diameter of which is ⅜″. Forty strips of brass 1-32″ thick, 1″ wide and 20″ long, are then drilled at the center and at the two ends with a 7-16″ hole, so as to admit the three bushed rods with a good deal of play. The strips are all acid blackened and slipped on the rods one at a time. They are inclined at an angle of 60°. A fiber bushing ½″ long, sawed at an angle of 60° is slipped on each of the rods between each of the metal strips. Before blackening, the strips should be carefully smoothed

on their upper surface. This grill when properly acid blackened reflects no light. At first sight objection may be raised to it for the reason that it slows the progress of the animal. This is a distinct gain rather than otherwise. It is distinctly more difficult for the animal to make a hurried dash to the food boxes. It forces leisure for choice. Since there are no sharp points the animals are not discommoded by it, and rapidly become accustomed to making the proper steps. The cover for the box should be made in a similar way, only in this case the metal strips need not be insulated. Furthermore, the strips may be separated from one another by a distance of 2″ or more. If small animals like the rat or mouse are used, a woven wire cover may be placed above the cover just described. After the animal has had a few days' experience in the box, the remaining tests may be made without the use of the cover.

(c). Ordinary high grade dead black *velveteen* may be used for lining the walls and partition of the experiment box. The addition of the velvet lining is merely a precautionary measure since the diaphragms effectually prevent all lateral reflection.

When the experiment box is used with the color apparatus, one other matter should be considered. It will be remembered that the experiment box has to be shifted 25 cm. each time the relative position of the two colors is reversed. It is just possible that such a movement of the experiment box might give the animal a secondary criterion for making a choice. In order completely to eliminate such a possibility it is well to make compartment A, fig. 4, entirely separate from B. Compartment A, containing the animal, may then be removed and the apparatus adjusted for a new test before A and B are joined.

VI. CONCLUSIONS AND SUMMARY OF RECOMMENDATIONS

The purpose of this investigation has finally turned out to be the devising of apparatus and modes of procedure by which highly accurate determinations may be made (a) of the limen (threshold) light and color stimuli for an animal; (b) of the limits of sensibility with respect to wave-length and intensity; (c) of the difference limen for light and for chromatic stimuli; (d) of the stimulating values of different chromatic stimuli (curve of luminosity for the eye of a given animal should thus be obtained); (e) of the chief characteristics of adaptation; (f) of visual defects in animals.

It is our conviction that the methods which are herein recommended will enable us to discover, by the comparative method, facts which are of fundamental importance for visual theory.

We desire to offer the following recommendations concerning methods of studying the various aspects of vision which have been considered in this report.

Light perception.—That the standardized light apparatus, as herein described or in improved form, be employed for accurate determination of the capacity of organisms to respond to light. In connection with the use of this apparatus we deem it important that tungsten lamps be used as sources of stimuli and as photometric and radiometric standards; that the light box be provided with movable screens which shall minimize the reflection within the light box; that the standard plates be accurately cut and kept in perfect condition; that differences in the visibility of portions of the two symmetrical halves of the experiment box, resulting from different intensities of the two stimuli or from differences in the surfaces of the parts of the box be excluded, as far as possible, by means of such devices as are suggested on page 85; that experiments be conducted in ordinary illumination whenever possible; that the experimenter to the extent of his ability strive to render the apparatus automatic in its operation so that he shall be able to avoid disturbing the reacting animal; that intensive painstaking observations with one or a very few individuals be sought in preference to extensive and superficially made observations on a large number of individuals; that a thorough investigation of the *light* vision of an animal preface and prepare the way for any investigation of its *color* vision; that the intensity of lamps used as sources be accurately determined to begin with and at stated intervals by means of reliable standards and measuring devices, such for example as the standardized tungsten lamp and the Lummer-Brodhun contrast photometer; that radiometric measurements be made with a radiomicrometer or a selenium cell; that stimulus areas be photometered by comparing them (balancing them against) a precisely similar standard area, 5 or 6 cm. circle, one meter distant, which in turn is illuminated by the standard source (tungsten). This standard source is to be mounted upon a movable carriage. It is further recommended that in arranging the apparatus for making

the determination the Lummer-Brodhun screen be fixed midway between stimulus area and standard area, and that the coarse adjustment (necessary to bring the intensity within the range of the photometer bar) be made by using a rotating sector (See p. 76) in the path of the light from the standard source, and that the fine adjustment be made by moving the standard lamp towards or away from the standard area until a balance is obtained in the Lummer-Brodhun screen. When on the other hand it is desired to set the stimulus lamps so as to give a known illumination value upon the stimulus area the standard lamp is set at the desired distance from the standard area and the stimulus lamp adjusted until the desired balance is obtained in the photometer. Every precaution must be taken to guard the screen from extraneous reflected light. It is recommended that this be done by the introduction of screens as described by C. H. Sharp (Measurement of light intensity, "Lectures on illuminating engineering," vol. 1, p. 411).

Size perception.—The recommendations under light perception apply here also. In addition, it is desirable that the standard plates be chosen so that discrimination shall at first be fairly easy; that after the ability to choose correctly on the basis of size has appeared check tests be devised which shall exclude possible peculiarities in the plates or in the illumination of the box; that differences in the illumination of the two halves of the experiment box, resulting from differences in the size of the stimulus areas be carefully controlled so that the animal shall not acquire a habit of attempting to discriminate on that basis; that the photometric values of each size stimulus be determined by balancing it against the light transmitted by a circle of standard area (6 cm. diameter, 28,2743 sq. cm. area) in fixed relation to standard source; that energy measurements be made with radiomicrometer or selenium cell, as in the case of light perception.

Form perception.—In addition to the above recommendations we would suggest the desirability of making certain that discrimination is dependent upon form alone; that plates of same area and plates whose opening may be inscribed within the standard 6 cm. circle be used in varied relations; that the relative ease of discrimination by form and size be determined in preliminary tests.

Distance perception.—It has not been possible for us to provide a method of testing this factor of visual discrimination with our standardized apparatus without troublesome and expensive complications. We, therefore, have left this aspect of vision for examination by means of devices which may be introduced in connection with our light and color apparatus. It is our plan to arrange mechanical devices by means of which the distance of the two stimulus areas from the animal may be altered by a few centimeters without avoidable changes in the other conditions. The testing of apparent distance as a factor in discrimination is especially important because of the distance-effects of colors.

Color perception.—The method of applying chromatic stimuli should be determined by (a) the manner in which the stimulus is obtained (reflection, transmission, or dispersion method) and (b) the characteristics of the animal. The first task of the investigator is to decide which of the three methods of obtaining stimuli will best serve his purposes. Having selected one of the three general methods, he should perfect an experiment box to use in connection with the standard light or color apparatus (the *light* apparatus may perfectly well be used in tests for color vision with either reflected or transmitted light from papers, cloths, glasses, etc.)

Our chief recommendations concerning the use of the color apparatus are these: That for high intensity work either the sun or an automatic open arc lamp, and for low intensity work a Nernst glower, be used as a source; that compound achromatic lenses be used for condenser, collimator, objective, and single achromats for small projector; that the double slit be calibrated for wave-lengths; that accurately cut and calibrated rotating sectors be used for the control of the intensity of the two beams, but that iris diaphragms be used instead, for supplementary check tests; that extreme pains be taken to prevent dependence upon secondary criteria, such as differences in illumination, differences in size, form, or apparent distance of stimulus areas; that in addition to making accurate photometrical and radiometrical measurements of the stimuli, the experimenter vary the conditions of the tests in all convenient ways in order to make certain of the kind of discrimination with which he is dealing.